EXPERIMENTS

AND

OBSERVATIONS

ON

ANIMAL HEAT,

AND THE

INFLAMATION

OF

COMBUSTIBLE BODIES.

BEING AN ATTEMPT

TO RESOLVE THESE PHÆNOMENA

INTO A

GENERAL LAW OF NATURE.

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M,DCC,LXXXVII.

TO

JOHN WATKINSON, M. D.

DISTINGUISHED BY

HIS SKILL IN THE ART OF MEDICINE,

BY HIS KNOWLEDGE IN

PHILOSOPHY AND POLITE LITERATURE;

AND BY THOSE

PRINCIPLES OF PROBITY AND HONOUR,

WHICH SECURE THE ATTACHMENTS OF FRIENDSHIP,

AND THE

CONFIDENCE OF SOCIETY,

THESE EXPERIMENTS

AND

OBSERVATIONS

ARE INSCRIBED,

BY HIS MOST SINCERE FRIEND,

AND OBLIGED HUMBLE SERVANT,

THE AUTHOR.

ADVERTISEMENT.

IT is proper to apprize the reader who would wiſh to repeat the experiments recited in the following pages, that ſuch experiments are liable to be affected by a variety of adventitious circumſtances, ſo minute as to require the moſt attentive obſervation. A change in the temperature of the air in the room, a variation in the time that is employed in mixing together the ſubſtances which are to have their comparative heats determined, a difference in the ſhape of the veſſel, or in the degree of agitation that is given to the mixture, will often produce a conſiderable diverſity in the reſult of the ſame experiment. It is poſſible however, by a ſeries of repeated trials, with well conſtructed thermometers, in the ſame circumſtances, to make a very near approximation to the truth, and, from a coincidence of facts, to obtain a degree of evidence, on which the mind may reſt with entire confidence.

Much time and labour have been employed in endeavouring to render the following experiments accurate: But if, after all, ſome miſtakes of leſs moment ſhould be diſcovered, it is hoped that the candour of the public will excuſe them, as the author is perſuaded, that the facts from which the general concluſions have been drawn, are well founded.

EXPERIMENTS

AND

OBSERVATIONS

UPON

ANIMAL HEAT, &c.

THE words *heat* and *fire* are ambiguous. Heat in common language, has a double ſignification. It is uſed indiſcriminately to expreſs a *ſenſation* of the mind, and an unknown principle, whether we call it a *quality* or a *ſubſtance*, which is the exciting cauſe of that ſenſation. *

By many ingenious philoſophers, in modern times, the word Heat has been applied to the unknown principle, and has been taken in a much greater extent than in common language. For, in common language, it is uſed to expreſs ſuch a degree of the unknown external cauſe, as will produce a given effect upon the ſenſes: but in the philoſophical acceptation,

* See Dr. Ried's Inquiry into the Human Mind.

tation, it expreſſes the external cauſe in the abſtract, without regard to the effects which it may produce. For the ſake of greater accuracy, in the following Diſſertation, I ſhall, with the ingenious Dr. Irvine of Glaſgow, call the latter *abſolute* heat; and what is denominated heat in common language, I ſhall call *ſenſible* heat.

From this view of the matter, it appears, that *abſolute* heat expreſſes that *power* or *element*, which, when it is preſent to a *certain* degree, excites in all animals the *ſenſation* of heat; and ſenſible heat expreſſes the ſame power, conſidered as *relative* to the *effects* which it produces.

Thus we ſay, that two bodies have equal quantities of ſenſible heat, when they produce equal effects upon the mercury in the thermometer; and that the ſame body has a greater or leſs degree of ſenſible heat, according as is produces a greater or a leſs effect upon this fluid.

But it will hereafter appear, that bodies of different kinds, have different capacities for containing heat; and, therefore, in ſuch bodies, the abſolute heat will be different, though the ſenſible heat be the ſame. If a pound of water, for example, and a pound of antimony, have the ſame temperature, we ſay that their ſenſible heats are equal: But we ſhall find, that the former contains a much greater quantity of abſolute heat than the latter.

Fire, in the vulgar acceptation of the word, expreſſes a certain degree of heat, accompanied with light; and is particularly applied to that heat and light which are produced by the inflammation of combuſtible bodies. But as heat, when accumulated in a ſufficient quantity, is conſtantly accompanied with light, or, in other words, as fire is always produced by the increaſe of heat, philoſophers have generally conſidered theſe phenomena as proceeding from

from the ſame cauſe; * and have, therefore, uſed the word *fire* to expreſs that unknown principle, which, when it is preſent to a certain degree, excites the ſenſation of heat alone, but when accumulated to a greater degree, renders itſelf obvious both to the ſight and touch, or produces heat, accompanied with light.

In this ſenſe, the element of fire ſignifies the ſame thing with abſolute heat.

Theſe definitions and remarks being premiſed, I ſhall proceed to give the reader a conciſe view of the general facts upon which the experiments, recited in the following pages, are founded.

1. Heat is contained in great quantities in all bodies, when at the common temperature of the atmoſphere.

In the deſerts of Siberia, as we learn from Baron Demidoff, the mercury ſometimes falls an hundred and fifty degrees below the freezing point. This is the moſt intenſe natural cold that has ever been known. But a much greater degree of cold has been produced by art. At Peterſburg, in the year 1759, the heat was ſo much diminiſhed by a mixture of ſnow and ſpirit of nitre, in the time of a ſevere froſt, that the ſpirit of wine thermometer ſunk to 148, and the mercurial thermometer to 352 degrees below the beginning of Farenheit's ſcale. As in this experiment the mercury was frozen, and as before its congealation, it contracted ſuddenly and irregularly, it has been concluded by Dr. Black and Dr. Irvine, that the cold which was then produced, was ſuch as was indicated by the ſpirit of wine thermometer; and, therefore, 148 degrees below 0 is conſidered as the freezing point of mercury. This is the greateſt cold that has ever been obſerved in nature;

* See Boerhaave, Martin M'Quer, &c.

nature; and yet we have no reafon to believe that the bodies which were expofed to it, were deprived of their whole heat. From thefe facts, however, we may conclude with certainty, that heat is contained in great quantities in all bodies, when at the common temperature of the atmofphere.

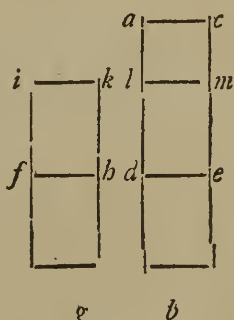
2. Heat has a conftant tendency to diffufe itfelf over all bodies, till they are brought to the fame degree of fenfible heat.

Thus, it is found by the thermometer, that if two bodies, of different temperatures, are mixed together, or placed contiguous, the heat paffes from the one to the other, till they both come to the fame temperature; and that all inanimate bodies, when heated, and placed in a cold medium, continually lofe heat, till, in procefs of time, they are brought to the ftate of the furrounding medium.

3. If the parts of the fame homogeneous body have the fame degree of fenfible heat, the quantities of abfolute heat will be proportionable to the bulk or quantity of matter. Thus the quantity of abfolute heat contained in two pounds of water, muft be conceived to be double of that which is contained in one pound, when at the fame temperature. This I think is evident from the fimilarity in the particles of the fame homogeneous folid and fluid fubftances. For the particles being fimilar, their powers will be equal; their capacities for receiving heat will be the fame; and therefore the quantities of abfolute heat which they contain, will be in proportion to the bulk or quantity of matter.

4. The mercurial thermometer is an accurate meafure of the comparative quantities of abfolute heat, which are communicated to the fame homogeneous bodies, or feparated from them, as long as fuch bodies continue in the fame form.

To illuſtrate this, let us ſuppoſe two equal parts of mercury, or of any other fluid. And let the part A be raiſed to a higher temperature than the part B, A will therefore contain a greater quantity of abſolute heat than B. Let them now be mixed; and according to the ſecond general fact, they will arrive at the ſame common temperature. But they cannot come to the ſame temperature, unleſs A communicates to B, one half of the exceſs of its heat above the original heat of B.



For, let the whole heat contained in A, previous to the mixture, be repreſented by the figure, *a b c*. And let the heat contained in B be repreſented by *f g h*. When they are mixed together and brought to the ſame temperature, let the heat of A be diminiſhed by the figure *a l m c*, and let that of B be increaſed by the figure *i f h k*. And ſince the heat which is taken from A is the very ſame with that which is added to B, it is manifeſt that *a l m c* muſt be equal to *i f h k*. Now it is affirmed, that *a l m c*, or *i f h k*, is equal to *l d e m*, or in other words, that A communicates to B one half of the exceſs of its heat above the original heat of B. For from the third general fact, it is evident, that when A and B are brought to the ſame temperature, they contain equal quantities of abſolute heat. Therefore the figure *i g k*, is equal to the figure *l m b*. For the ſame reaſon the figure *f g h*, is equal to the figure *d b e*. And therefore the remainder *i f h k*, or *a l m c*, is equal to the remainder *l d e m*.

If, therefore, a mercurial thermometer, of the ſame temperature with a quantity of water, the ab-

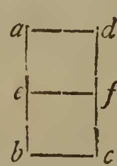
ſolute heat of which is repreſented by *d b e*, be immerſed in the ſame water, when its abſolute heat is repreſented by *l b m*, and be again immerſed in it, when its abſolute heat is repreſented by *a b c;* and if the expanſions of the mercury be in theſe inſtances, in the ratio of one to two, it is evident that the expanſions, and the increments of abſolute heat, will be in exact proportion.

A variety of experiments were made upon this principle by Monſ. De Luc, from which it appears, that the expanſions of mercury between the freezing and boiling points of water, correſpond preciſely to the quantities of abſolute heat applied.

The mercurial thermometer therefore is an accurate meaſure of the comparative quantities of abſolute heat, which are communicated to the ſame homogeneous bodies, or ſeparated from them, as long as ſuch bodies continue in the ſame form.

It has been already ſhown, that in the ſame homogeneous bodies, if the quantities of matter be different, but the temperatures the ſame, the quantities of abſolute heat will be, in proportion to the quantities of matter.

It now appears, that in the ſame homogeneous bodies, if the temperatures be different, but the quantities of matter the ſame, the quantities of abſolute heat will be in proportion to the temperatures, or to the expanſions in the mercurial thermometer.



For let the whole of the abſolute heat contained in a pound of ice, at the temperature of 0, the beginning of Farenheit's ſcale, be repreſented by the figure, *a b c d*, and let this heat be diminiſhed till it becomes equal to *e b c f*, which is the one half of *a b c d*. If a mercurial thermometer were deprived of its whole heat,

and

and applied to the ice when its heat is reprefented by *e b c f*, and were again applied to the fame ice when its heat is reprefented by *a b c d;* the expanfion produced by the heat *e b c f*, would be to that produced by *a b c d*, in the ratio of one to two*. If *a b c d* were tripple of *e b c f*, the expanfions would be in the ratio of one to three; and, univerfally, the temperatures being different, but the quantity of matter the fame, the quantities of abfolute heat will be in proportion to the temperatures, as meafured by the mercurial thermometer.

This conclufion is an inference from De Luc's experiments, which prove, as was obferved above, that the expanfions of mercury are in proportion to the increafe of the abfolute heat, and its contractions to the diminution of this element, in all the intermediate degrees, between the boiling and freezing points of water: from whence it is inferred, that if the mercury were to retain its fluid form, its contractions would be proportionable to the decrements of the abfolute heat, though the diminution were continued to the point of total privation.

Corollary. If therefore the fenfible heat of a body, as meafured by the mercurial thermometer, were to be diminifhed the one half, or the one third, or in any given proportion, the abfolute heat would be diminifhed in the fame proportion.

5. The comparative quantities of abfolute heat which are communicated to different bodies, or feparated from them, cannot be determined in a direct manner by the thermometer. Thus if the temperature of a pound of mercury be raifed one degree, and that of a pound of water one degree, as indicated by the thermometer, it does not by any means

 follow,

* It is here fuppofed that the mercury continues fluid when it is deprived of its whole heat.

follow, that equal quantities of abfolute heat have been communicated to the water and the mercury.

It has been thought by fome philofophers, that the quantities of abfolute heat in bodies, are in proportion to their denfities. Boerhaave was of opinion, that heat is equally diffufed thro' all bodies, the denfeft as well as the rareft, and therefore that the quantities of heat in bodies are in proportion to their bulk.

But it appears from experiment, that the law of the diftribution of heat throughout the various claffes of natural bodies, is not according to the ratio either of *bulk* or *denfity*.

The firft attempt to determine by experiment the comparative quantities of abfolute heat in bodies, was made by Farenheit, at the defire of Boerhrave.

The following is a fhort fketch of this attempt, nearly in the words of the author:

If you take equal quantities of the fame fluid, and give them different degrees of heat, and mix them intimately together, the temperature of the mixture will be half the excefs of the hotter above the colder,

If, for example, a pint of boiling water at 212, be mixed with a pint of the fame fluid at 32, the temperature of the mixture will be 122: the warm water will be cooled 90 degrees, and the cold water heated 90.

But if this experiment be made with water and mercury, in the fame circumftances, the effect will be very different.

For if you take equal bulks of mercury and water, and give the water a greater degree of heat than the mercury, the heat of the mixture will always be greater than half the excefs of the heat of the water above that of the mercury.

On

On the other hand, if the mercury be hotter than the water, the temperature of the mixture will conſtantly be leſs than half the exceſs of the heat of the mercury above that of the water. The changes which are produced in the temperature of the water and mercury, in the firſt of theſe inſtances, are found to correſpond to thoſe which are produced, by mixing three parts of hot water with two of cold; and in the ſecond inſtance, to thoſe which take place, when three parts of cold water are mixed with two of hot. That is, the change produced in the heat of the mercury, is to that produced in the heat of the water, as three to two.

From the former of theſe experiments it was juſtly concluded by Boerhaave, that in the ſame body, the diſtribution of fire is in proportion to the bulk or quantity of matter.

But from the experiments with water and mercury, he concluded very unwarrantably, that heat is equally diffuſed thro' all bodies, the denſeſt as well as the rareſt; and therefore that the quantities of heat in different bodies, are in proportion to their bulks, or to the ſpaces which they occupy.

Theſe experiments have been repeated and varied in the preſent age, and very different concluſions have been drawn from them.

I have already obſerved, that, if a pint of mercury at 100, be mixed with an equal bulk of water at 50, the change produced in the heat of the mercury, will be to that produced in the heat of the water, as three to two; from which it has been inferred, that the abſolute heat of a pint of mercury is to that of an equal bulk of water, as two to three: or, in other words, that the comparative quantities of their *abſolute* heats are reciprocally proportionable to the changes

ges which are produced in their *ſenſible* heats, when they are mixed together at different temperatures *.

The truth of this concluſion may be illuſtrated in the following manner.

If ~~four~~ three pounds of diaphoretick antimony at 20, be mixed with one pound of ice at 32, the temperature of the mixture will be very nearly 26. The ice will be cooled ſix degrees, and the antimony heated ſix. If we reverſe the experiment, the effect will be the ſame. That is, if we take ſix degrees of heat from three pounds of antimony, and add it to a pound of ice, the latter will be heated ſix degrees. The ſame quantity of heat, therefore, which raiſes a pound of ice ſix degrees, will raiſe three pounds of antimony ſix degrees.

If this experiment be made at different temperatures, we ſhall have a ſimilar reſult. If, for example, the antimony at 15, or at any given degree below the freezing point, be mixed with the ice at 32, the heat of the mixture will be half the exceſs of the hotter above the colder. From hence we infer, that the reſult would be the ſame, if the antimony were deprived of its whole heat, and were mixed with the ice at 32. But it is evident, that, in this caſe, the ice would communicate to the antimony the half of its abſolute heat †. For, if 200 below froſt, be conceived to be the point of total privation, the antimony will be wholly deprived of its heat, when its temperature is diminiſhed 200 degrees below 32; and the heat contained in the ice, when at

* This fact has, for ſeveral years, been taught publicly by Dr. Black and Dr. Irvine, in the Univerſities of Edinburgh and Glaſgow: and it has been applied by Dr. Irvine, to the ſolution of a variety of curious and important phenomena.

It is hoped, that thoſe learned and ingenious philoſophers will ſoon favour the world with their reſpective diſcoveries in this branch of ſcience.

† See the Corollary to the 4th general fact.

at 32, will be 200 degrees. If we now ſuppoſe them to be mixed together, the temperature of the mixture will be half the exceſs of the hotter above the colder; or the ice will be cooled 100 degrees, and the antimony heated 100. The one half of the heat, therefore, which was contained in the ice, previous to the mixture, will be communicated to the antimony: from which it is manifeſt, that, after the mixture, the ice and antimony muſt contain equal quantities of abſolute heat.

To place this in another light, it has been proved, that the ſame quantity of heat which raiſes a pound of ice ſix degrees, will raiſe three pounds of antimony ſix degrees. From which it is inferred, that the ſame quantity of heat which raiſes the ice 200 degrees, or any given number of degrees, will raiſe the antimony an equal number of degrees *.

A pound of ice, therefore, and three pounds of antimony, when at the ſame temperature, contain equal quantities of abſolute heat. But, it appears from the third general fact, that three pounds of antimony contain three times as much abſolute heat, as one pound of antimony; and hence the abſolute heat of a pound of ice, is to that of a pound of antimony, as three to one.

Again, if a pound of ice at 32, be mixed with a pound of antimony at 12, the temperature of the mixture will be 27; the ice will be cooled five degrees, and the antimony heated 15; or the change produced in the ſenſible heat of the ice, will be to that produced in the ſenſible heat of the antimony, as one to three. But it was before proved, that the abſolute heat of a pound of ice, is to that of a pound of antimony, as four to one. From which it is evident, that the comparative quantities of abſolute heat, in

* It is here ſuppoſed, that the ice continues in the ſame form.

in equal weights of ice and antimony, are reciprocally proportionable to the changes which are produced in their ſenſible heats, when they are mixed together at different temperatures *.

Thus it appears, that the comparative quantities of abſolute heat in bodies may be determined, by mixing them together as above, and obſerving the changes which are produced in their ſenſible heats. This rule, however, does not apply to thoſe ſubſtances, which, in mixture, excite ſenſible heat or cold by chymical action.

From the reaſoning which was employed to eſtabliſh the above propoſition, it follows, that equal weights of hetrogeneous ſubſtances, as air and water, having the ſame temperature, may contain unequal quantites of abſolute heat. There muſt, therefore, be certain eſſential differences in the nature of bodies, in conſequence of which *ſome* have the power of collecting and retaining the element of fire, in greater quantities than *others*. Theſe different powers are called in the following pages, the *capacities* of bodies for containing heat. Thus, if we find by experiment, that a pound of water contains three times as much abſolute heat, as a pound of antimony, the capacity of water for containing heat, is ſaid to be to that of antimony, as three to one.

SECT.

* I have thus endeavoured briefly to eſtabliſh the truth of the above doctrine, as a neceſſary introduction to the experiments which follow, but the more full and complete illuſtration of it I ſhall leave to Dr. Black and Dr. Irvine.

This diſcovery opens a wide field for inveſtigation, as by means of it we are enabled to eſtimate the comparative quantities of abſolute heat in bodies, and to determine with certainty and accuracy, the various proportions in which the element of fire is diſtributed throughout the different kingdoms of Nature.

S E C T. II.

HAVING premifed thefe general facts, I fhall now lay before the reader my Experiments on Animal Heat, and the Inflammation of Combuftible Bodies. *

It was obferved above, that fenfible heat has a conftant tendency to diffufe itfelf equally over all bodies, till they are brought to the fame temperature. From this property of heat, it is manifeft, that thofe animals which have a higher temperature than the medium in which they live, muft be continually communicating heat to the furrounding bodies. Since therefore, in the animal kingdom, there is a conftant diffipation of heat, it follows, that there muft be a proportionable fupply of this element, to repair the wafte. For, if the animal body had not the power of exciting or collecting heat, it would foon arrive at the temperature of the ambient medium.

With a view to difcover the nature of this power, I made a variety of experiments, in the fummer, 1777, on animal, vegetable, and mineral fubftances;

fome

* I think it proper to obferve, that I was led to the confideration of this fubject, in confequence of having attended the chymical Lectures of the learned and ingenious Dr. Irvine of Glafgow. And it is a tribute of juftice which I owe to this philofopher, to acknowledge, that the folution which he has given of Dr. Black's celebrated difcovery of latent heat, or of the heat which is produced by the congelation of water, fpermaceti, bees-wax, metals, &c. fuggefted the views which gave rife to my experiments.

I muft alfo add, that Dr. Irvine, from the folution which he has given of the above phenomena, and from the general fact, that heterogeneous bodies have different capacities for containing heat, concluded, that there was a difference between the abfolute heat of fixed and atmofpherical air. It remained to afcertain this difference, and to determine by experiments, whether fixed or atmofpherical air, contained the greater quantity of abfolute heat.

fome of which I fhall now relate, as I think they have led to the true fource, from whence the heat of animals, and the heat which is produced by the inflammation of combuftible bodies, is derived.

I muft firft obferve, that experiments for determining the comparative quantities of abfolute heat in bodies, are liable to three caufes of inaccuracy. 1. When the fubftances to be compared, are mixed together, a certain time is required for the heat to pafs from the warmer to the colder body, till they arrive at the fame temperature. If they be *intimately* mixed, and the veffel be a little agitated, a minute is generally fufficient. During this interval, a part of the heat is carried off by the furrounding atmofphere. It therefore becomes neceffary, to calculate the heat which is thus loft in the firft minute.

A very fimple and ingenious rule was laid down for this purpofe, by the illuftrious Sir Ifaac Newton.

Confidering the heat in a body, as the excefs whereby it is warmer than the furrounding medium, he fuppofed, that the quantities of heat loft in fmall portions of time, would always be proportionable to the heats remaining. Thus, if a body were 180 degrees hotter than the atmofphere, the quantity of heat which it would lofe in a given moment, would be double of that which it would lofe in an equal portion of time, if it were only 90 degrees hotter than the atmofphere. And, therefore, if the times be taken in arithmetical progreffion, the decrements of heat will be in geometrical progreffion. But, it has been obferved by Dr. Martine, that this rule is not to be admitted without fome reftriction. He has inferred from a variety of experiments, that the differential decrements are in a proportion fomewhat greater than the inherent quantities of heat; and, there-

therefore, that the quantities of heat loſt, are partly equable, and partly in geometrical progreſſion*.

This obſervation of Dr. Martine, it muſt be allowed, is well founded. But when the body, which is to be the ſubject of an experiment, tranſmits heat very faſt, and its temperature is much greater than that of the ambient medium, the error produced, by calculating according to Sir Iſaac Newton's rule, is ſo very inconſiderable, that it may be neglected.

On the contrary, when the experiment is made in a veſſel that tranſmits heat very ſlowly, and the heat of the ſubſtance to be examined is not much greater than that of the atmoſphere, the quantities of heat loſt in any two ſmall ſucceſſive portions of time, approach ſo nearly to an equality, that the difference cannot be diſtinguiſhed by the niceſt thermometer. That this obſervation is well founded, appears from the following experiment:

A pound of water in an earthen veſſel, being raiſed to 120, the temperature at the end of

1 minute was	—	—		119
2	—	—	—	118
3	—	—	—	117
4	—	—	—	116
5	—	—	—	115
6	—	—	—	114
7	—	—	—	113

In the experiments which I ſhall hereafter relate, the order of cooling was obſerved for ſeveral minutes; and, in general, the heat which was loſt in the firſt minute, was calculated according to Sir Iſaac Newton's rule, from the ſeries of numbers determined by obſervation.

I muſt here obſerve, that I believe Dr. Irvine was the firſt who applied this rule of Sir Iſaac Newton,

* See Martine's Eſſays.

to calculate the heat loſt during the firſt minute, in experiments for determining the abſolute heat of bodies.

2. If, in making theſe experiments, the ſubſtance which has the greateſt heat, be mixed with that which has the leaſt, in a cold veſſel, a part of the heat will be communicated to the veſſel: But if the experiment be made in a warm veſſel, the cold ſubſtance will receive heat, not only from the warm ſubſtance, but from the veſſel in which it is contained.

The moſt effectual method of avoiding this cauſe of inaccuracy, is firſt to determine the capacity of the veſſel for receiving heat, compared with that of one of the ſubſtances to be examined.

The relative capacity of the veſſel, in which ſome of the following experiments were made, compared with that of water, were thus determined.

The air in the room being —	61
A pound of water at — —	168
was poured into an earthen veſſel at —	68
The temperature of the water at the end of	
1 minute was — —	155
2 — — —	150
3 — — —	145

To diſcover the heat communicated to the atmoſphere in the firſt minute, ſay as 84 to 89 ſo is 94 to a fourth proportional, which gives 99.5. From which it appears that 5.5 were carried off by the air in the firſt minute, adding 5.5 to 155 we have 160.5 for the true temperature of the water and of the veſſel.

Subtracting this from 168, we have 7.5 for the quotient. The water was therefore cooled by the veſſel 7.5, and the veſſel was heated by the water 92.5. And ſince the veſſel received this heat from the water, it is manifeſt, that the ſame quantity of heat,

heat, which changes the temperature of a pound of water, 7.5 will change the temperature of the veffel 92.5. And by a parity of reafoning, the fame heat which raifes the water one degree, will raife the veffel $12\frac{1}{3}$. If, therefore, in any experiment, we find that the veffel has received $12\frac{1}{3}$ of heat from a pound of water, we may be fure that the feparation of this heat, has cooled the water one degree.

3. In experiments for determining the abfolute heat of bodies, water is generally the ftandard. When the body which is to have its heat compared with that of water, tranfmits heat very flowly, it frequently happens, that the different parts of the mixture, cannot be brought precifely to the fame temperature for feveral minutes. Thus I find that in experiments upon vegetables and the calces of metals, there is a confiderable difference, at the end of the firft minute, between the heat of the mixture at the furface, and that at the bottom of the veffel; arifing partly from the tendency of the warmeft part of the water to remain at the furface, and partly from the difficulty with which the above-mentioned fubftances tranfmit heat. This may be remedied in fome degree, by mixing the bodies together intimately, and agitating the mixture brifkly. But by much agitation, the heat is carried off fuddenly and irregularly, which makes it difficult to calculate the heat that is loft in the firft minute.

The method which I apprehend, leaft liable to error, is to agitate the mixture moderately, and at the end of the firft minute, to take the arithmetical mean between the heat at the furface, and that at the bottom of the veffel; and if in a variety of experiments, in different circumftances, we find that the refult is nearly the fame, we may be fure that we have approached very near the truth.

With theſe precautions, the following experiments were made, to determine the abſolute heat of ſome of the moſt common vegetable and animal ſubſtances, compared with that of water.

EXPERIMENT I.

Air in the room	— —	69
A pound of wheat at	—	66
was mixed with a pound of water at		166

The mixture being agitated for a ſhort time,
The temperature at the end of

	furface		bottom		medium
1 minute was	138	—	128	—	133
2 —	134	—	125	—	129½
3 —	131	—	122	—	126½
4 —	127	—	120	—	123½

The mean temperature of the mixture at the end of one minute was 133, at the end of two minutes 129½, at the end of three minutes 126½: And therefore the heat carried off by the air in the firſt minute, being calculated according to Sir Iſaac Newton's rule, was very nearly 3½. If we add this to 133, we ſhall have 136½ for the true temperature of the mixture. It was proved that the capacity of the veſſel for receiving, heat, was to that of the water as 1 to 12⅕. In the experiment which we are now conſidering, the veſſel was raiſed from 66 to 133, or 67.5; dividing this by 12⅕, we have nearly 5.5 for the quotient. From which it appears, that the water was cooled by the veſſel 5.5, or the veſſel ſeparated 5.5 degrees from the water. The true temperature of the mixture was 136½, ſubtracting this from 166, we have 29½ for the remainder. The water was therefore cooled 29½ by the wheat and the veſſel together. But we have ſhown that it was cooled 5.5 by the veſſel; it was therefore cooled 24 by the wheat. But the wheat was raiſed from 66 to 136½ or 70.5. It follows,

follows, that the fame quantity of heat which will change the temperature of water 24, will change that of wheat 70½. Therefore the abfolute heat of water is to that of wheat, as 70½ to 24, or very nearly as 2.9 to 1.

EXPERIMENT II.

Air in the room — — 60.
One pound of oats, having the hulls taken off, at — — 61,
was mixed with one pound of water, at 161;
The temperature of the mixture at the end of

	furface		bottom		medium
1 minute was	127	—	123	—	125
2 —	125	—	122	—	123½
3 —	121	—	121	—	121
4 —	118	—	118¾		
5 —	114	—	116		
6 —	111	—	115		

To 125, the mean temperature of the mixture, adding 2½ for the heat carried off by the air in the first minute, we have 127½ the true temperature of the mixture.

By calculating, as in the preceding experiment, it appears that the water was cooled by the veffel nearly 5.4. The oats were raifed from 61 to 127½ or 66½; fubtracting 127½ from 161, we find that the water was cooled by the oats and the veffel together 33½; but it was cooled 5.4 by the veflel; it was therefore cooled 28.1 by the water: and hence the abfol[illegible] heat of water is to that of oats, as 66.5 to 28. , o as 2.36 to 1, that is, nearly, as 2⅓ to 1.

EXPERIMENT III.

Air in the room — 59.
One pound of beans at — 60,
was mixed with one pound of water at 160

The

The temperature of the mixture at the end of

	furface		bottom		medium
1 minute, was	119	—	113	—	116
2 —	117	—	109	—	113
3 —	116	—	107	—	111½
4 —	113	—	106	—	109½
5 —	111	—	105	—	108
6 —	109½	—	104½	—	107
7 —	107½	—	104	—	106¾
11 —	101	—	101	—	101

To 116 adding 3 degrees for the heat carried off by the air in the firft minute, we have 119 for the temperature of the mixture. It appears, from calculation, that the water was cooled by the veffel 4.8. The beans were raifed from 60 to 119, or 59 degrees, fubtracting 119 from 160, we find that the water was cooled, by the beans and the veffel together, 41 degrees. But it was cooled by the veffel 4.8. It was therefore cooled by the beans 36.2. And hence the abfolute heat of water is to that of beans as 59 to 36.2, or nearly as 1.6 to 1.

EXPERIMENT IV.

Air — — — 61.
A pound of barley at — — 60,
was mixed with a pound of water at 160;
The temperature of the mixture at the end of

	furface		bottom		medium
1 minute was	126	—	120	—	123
2 —	122	—	115	—	118½
3 —	119	—	113	—	116
6 —	109	—	109	—	109

To 123, the mean temperature, adding 4.½ for the heat carried off by the air in the firft minute, we have 127½ the true temperature of the mixture. The veffel was raifed by the water 66½, dividing this by 12⅓, we have 5.3 for the quotient; by which it appears

appears that the water was cooled by the veſſel 5.3. The barley was raiſed from 60 to $127\frac{1}{2}$ or $67\frac{1}{2}$. The water was cooled by the veſſel and the barley together, from 160 to $127\frac{1}{2}$, or $32\frac{1}{2}$. But it was cooled 5.3 by the veſſel. It was therefore cooled 27.2 by the barley: and hence the abſolute heat of water is to that of barley as 67.5 to 27.2, or nearly as 2.4 to 1.

EXPERIMENT. V.

Air — — 60.
One pound of the lungs of a ſheep at 59,
was mixed with a pound of water at 149;
The mixture ar the end of

	ſurface		bottom		medium
1 minute was	112	—	108	—	110
2 —	109	—	106	—	$107\frac{1}{2}$
3 —	106	—	105	—	$105\frac{1}{2}$
4 —	103	—	103	—	103

Adding 2.5 for the heat carried off by the air in the firſt minute, we have 112.5 for the true temperature of the mixture,

The veſſel was raiſed from 59 to 112.5, or 53.5. Dividing this by $12\frac{1}{7}$,* we have nearly 4.3 for the quotient. From which it appears, that the water was cooled by the veſſel 4.3. The fleſh was raiſed from 59 to 112.5, or 53.5. The water was cooled by the fleſh and the veſſel, from 149 to 112.5, or 36.5; but it was cooled 4.3 by the veſſel; it was, therefore, cooled 32.2 by the fleſh. And hence the abſolute heat of water is to that of fleſh, as 53.5 to 32.2; or, nearly as 1.3 to 1.

EXPERIMENT VI.

Air — — 60,
One pound of milk at — — 60.
was mixed with a pound of water at 160;

The

The temperature at the end of

	surface		bottom		medium
1 minute was	109	—	107	—	108
2 —	106	—	104	—	105
3 —	103	—	102	—	102½

To 108, the mean temperature, adding 2.8, we have 110.8, the true temperature of the mixture.

The vessel was raised by the water 50.8. The water was therefore cooled by the vessel 4.1. But it was cooled by the milk and the vessel together 49.2. It was consequently cooled by the milk 45.1.

The milk was raised from 60 to 110.8, or 50.8. And hence the absolute heat of water is to that of milk, as 50.8 to 45.1; or, nearly as 1.1 to 1.

EXPERIMENT VII.

Half a pound of water at — 47,
was mixed with a half pound of blood at 98;

Having agitated the vessel, the heat was in a short time nearly equally diffused over the whole, and the mixture continuing fluid for the space of two minutes, its temperature at the end of

1 minute was —	71	
2 — — —	70	coagulated.
3 — —	70	
4 — —	70	
5 — — —	70	
6 — —	69	

The blood, which was the subject of this experiment, was procured from a sheep, by dividing the veins and arteries of the neck; and appeared by its colour to be a mixture of venous and arterial blood, though consisting chiefly of the latter. The capacity of the vessel in which this experiment was made, for containing heat, was to that of the water, as 6 to 94; or, nearly as 1 to 15.6.

The

The mixture cooled (if we except the time of its coagulation) at the rate of one degree in a minute. Adding one degree for the heat lost in the first minute, we have 72 for the heat of the mixture. The vessel was raised by the blood, from 47 to 72, or 25 degrees. The blood was therefore cooled by the vessel nearly 1.6: it was cooled by the water and the vessel together 26 degrees. It was therefore cooled by the water 24.4. The water was raised 25 degrees. And hence the absolute heat of a mixture of venous and arterial blood is to that of water, as 25 to 24.4.

These experiments prove, in general, that flesh, milk, and vegetables, contain less absolute heat than water, and water less than blood. Blood, therefore, contains a greater quantity of absolute heat, than the principles of which it is composed.

The remarkable accumulation of heat in this fluid, led me to suspect, that it absorbs heat from the air, in the process of respiration. And in this suspicion, I was much confirmed by the following considerations:

1. Those animals which are furnished with lungs, and which continually inspire the fresh air in great quantities, have the power of keeping themselves at a temperature considerably higher than the surrounding atmosphere. But animals that are not furnished with respiratory organs, are very nearly of the same temperature with the medium in which they live.

2. Among the hot animals, those are the warmest, which have the largest respiratory organs, and which consequently breathe the greatest quantity of air in proportion to their bulk. Thus, the respiratory organs of birds, compared with their size, are more extensive than those of any other animal; and birds have the greatest degree of animal heat.

 3. In

3. In the ſame animal, the degree of heat is in ſome meaſure proportionable to the quantity of air inſpired in a given time. Thus, we find that animal heat is increaſed by exerciſe, and by whatever accelerates reſpiration.

From theſe conſiderations, I was naturally led to a more particular examination of this ſubject: the reſult of which, is comprehended in the following propoſitions:

PROPOSITION I.

Atmoſpherical air contains a greater quantity of abſolute heat, than the air which is expired from the lungs of animals; and the quantity of abſolute heat contained in any kind of air that is fit for reſpiration, is very nearly in proportion to its purity, or to its power in ſupporting animal life.

Before I proceed to the direct proof of this propoſition, it is neceſſary to conſider the nature of the change which the air undergoes in the lungs. It is well known, that the air expired from the lungs, occaſions a precipitation in lime water. A part of it, therefore, conſiſts of fixed air. It has been found, by Dr. Prieſtley, that the reſiduum of this air, is a mixture of atmoſpherical, and what he has called phlogiſticated air, a ſpecies of air which occaſions no precipitation in lime water, but which extinguiſhes flame, and is noxious to animal life.

That the fixed air produced in reſpiration, depends upon a change, which the atmoſpherical air undergoes in the lungs, is, I think, evident, from the following facts.

Air is altered in its properties by phlogiſtic proceſſes, and though many of theſe proceſſes are totally different from each other, yet the change produced in the air, is in all caſes very nearly the ſame. It is diminiſhed in its bulk. It is rendered incapable of

main-

maintaining flame, and of ſupporting animal life. And, if we except a very few inſtances, where the fixed air is abſorbed, it univerſally occaſions a precipitation in lime water. We have therefore reaſon to believe, that there is no inſtance of a phlogiſtic proceſs in nature, which is not accompanied with the production of fixed air.

It may be ſuppoſed, indeed, that this air is diſcharged from the ſubſtance which furniſhes the phlogiſton.

In *ſome* caſes, a part of the fixed air may proceed from this ſource, as in various inſtances of combuſtion and putrefaction. But there are other caſes, in which we are certain, that the fixed air is derived from a change produced in the atmoſpherical air.

Thus, air diminiſhed by the burning of alcohol and ſulphur, occaſions a precipitation in lime water. The ſame effect is produced, when atmoſpherical air is diminiſhed by nitrous air, and when it is exploded with inflammable air; and yet it is not found that brimſtone, alcohol, nitrous or inflammable air, contain fixed air.

But Dr. Prieſtley has given us the moſt deciſive proof of this fact, in the following experiment. Having cauſed the electric ſpark to paſs through a glaſs tube, the lower part of which contained ſome water tinged with turnſol, he obſerved, that the blue colour of the liquor was, in a few minutes, changed to red, and that the included air was diminiſhed in its bulk, and rendered highly noxious. He likewiſe obſerved, that when the ſpark was taken in air over lime water, the lime was precipitated*.

Since, therefore, a quantity of fixed air, is, in theſe proceſſes, produced by a change in the atmoſpherical air, we may conclude by induction, that the

* See Dr. Prieſtley's experiments and obſervations upon air, Vol. I.

the ſame effect is produced in every other phlogiſtic proceſs.

Thus it appears, that in reſpiration, atmoſpherical air is converted into fixed and phlogiſticated air. It is therefore neceſſary, in order to determine the truth of the propoſition, that we ſhould compare the abſolute heat of fixed and phlogiſticated air, with that of atmoſpherical air.

The following experiments were made to determine the heat of theſe different ſpecies of air.

EXPERIMENT I.

Air in the room — 52.

A bladder containing a pint of atmoſpherical air at 102, was immerſed in a pint of water at 52.

The heat of the water at the end of

	ſurface	bottom
1 minute was	53	52¼
2	53	52½
3	53¼	53
4	53¼	53¼

In the above experiment, the air and the water do not ſeem to have been brought to the ſame temperature, till the end of 4 minutes. That this was really the caſe, is proved by the following experiment.

EXPERIMENT II.

Air in the room 64. A pint of water was taken, the temperature of which, was 63 at the ſurface, and 62⅓ at the bottom. A pint of atmoſpherical air confined in a bladder, was raiſed to 163. The bladder containing the air being immerſed in the water the heat was determined by two thermometers, one of which was placed near the ſurface of the water, in contact with the bladder and the other near the bottom.

At

At the end of 1 minute the thermometer at the furface was $67\frac{1}{2}$, thermometer at the bottom $62\frac{1}{3}$

2	minutes	65	—	63
3	—	65	—	$63\frac{1}{3}$
4	—	$64\frac{1}{2}$	—	$63\frac{2}{3}$
5	—	$64\frac{1}{3}$	—	$63\frac{2}{3}$

In this experiment, which was made with the fame bladder that was ufed in the former, the heat of the air in the bladder near the furface, exceeded the heat of the water for feveral minutes. And hence we may perceive the reafon why, in the firft experiment, the temperature of the water rofe gradually at the furface and the bottom, till the end of 4 minutes; for during that time, it continued to receive heat from the air which had been immerfed in it.

In that experiment, the heat communicated to the water by the air, and the bladder which contained it, was $1\frac{1}{4}$. Of this quantity of heat, the portion which was yielded by the bladder, will be feen by the experiment which follows:

EXPERIMENT III.

A pint of water was taken at 52. The bladder having been dried and freed from air, was raifed to 102, and being immerfed in the water, the heat of the water at the end of

	furface	bottom
1 minute was	$52\frac{1}{4}$	52
2 —	$52\frac{1}{4}$	$52\frac{1}{4}$

We muft allow, therefore, one quarter of a degree, in the experiment in queftion, for the heat imparted by the bladder. And hence it follows, that *one* degree was communicated by the air.

To difcover, from the above experiment, the abfolute heat of atmofpherical air compared with that of water, we may confider what would have been the effect produced, if atmofpherical air contained the

fame

ſame abſolute heat with water. In that caſe, if the air were only the one hundredth part as denſe as water, and were raiſed 100 degrees above the temperature of the water, it would communicate to it nearly one degree of heat. If it were only the one eighth hundredth part as denſe, and were raiſed 100 degrees above the heat of the water, it would communicate to it nearly the one eighth part of a degree. If it were raiſed only 50 degrees above the heat of the water, it would communicate the one ſixteenth part of a degree.

Now, the denſity of atmoſpherical air is to that of water, in a proportion ſomewhat leſs than that of 1 to 800. If, therefore, in the experiment in queſtion, the atmoſpherical air had contained the ſame abſolute heat with water, it would have communicated to the water, nearly the one ſixteenth part of a degree of heat. But it communicated to it one entire degree of heat. Atmoſpherical air muſt therefore contain at leaſt 16 times as much abſolute heat as water.

Dr. Irvine has pointed out a general rule, by which the comparative quantities of abſolute heat in bodies may be eſtimated, when the quantities of matter, and the changes produced in the ſenſible heats, are unequal. In that caſe, the quantities of abſolute heat, are reciprocally as the changes in the ſenſible heats, multiplied into the quantities of matter.

By the help of this rule, the ratio of the heat of atmoſpherical air to that of water, may be more accurately calculated in the following manner.

It is evident, that, as equal bulks of air and water were taken, if the denſities of the water and air had been equal, the abſolute heat of the air would have been to that of the water, as 1 to 49, which

is

is the reciprocal proportion of the changes produced in the fenfible heats.

Again, if the changes produced in the fenfible heats had been equal, the abfolute heats would have been reciprocally as the quantities of matter. It follows, that neither being equal, the abfolute heats are in the compound ratio, of the fenfible heat gained by the water, to that feparated from the air, and of the quantity of water to that of the air.

The experiment was made in a quart pewter veffel, the capacity of which for receiving heat was to that of the water very nearly as 1 to 16. The quantity of water was 16 ounces; and therefore, the heat received by the veffel was equal to that which would have been received by the one fixteenth part of 16 ounces, or by one ounce of water. The water and veffel together were confequently equal to 17 ounces of water; and the fpecific gravity of air being to that of water in the exact proportion of 1 to 862; it follows that the quantity of matter contained in 17 ounces of water is to that contained in a pint of air, nearly as 915 to 1. Hence the abfolute heat of air is to that of water in the compound ratio

of	915	to	1, and
of	1	to	49

From which it appears, that the quantity of heat contained in the former of thefe elements, is to that contained in the latter, as 915 to 49, or nearly, as 18.6 to 1.

In calculating this experiment, no allowance was made for the heat carried off by the external air; for as the temperature of the water, after the air was immerfed in it, exceeded that of the atmofphere, only $1\frac{1}{4}$, the portion of heat which was thus carried off was fo very fmall, that it may be neglected.

 I muft

I muſt farther obſerve, that, from the imperfection of thermometers, and from the difficulty of judging by the eye of one fourth or one third of a degree, perfect accuracy in experiments of this kind is not to be expected.

The experiment, however, for determining the heat of atmoſpherical air, has been frequently repeated--every repetition has tended to confirm the general concluſion, and from the reſult of a variety of trials, I have the greateſt reaſon to believe that the ratio of the heat of atmoſpherical air to that of water, as deduced from the above experiment, does not exceed the truth.

I propoſe, in future, to endeavour to aſcertain, with as much accuracy as poſſible, the heat of the different ſpecies's of air, by a greater variety of trials, and by thermometers conſtructed for the purpoſe

I next proceed to determine the heat of fixed and phlogiſticated air.

EXPERIMENT IV.

Air in the room 52. A pint of fixed air, extricated from chalk by the vitriolic acid, was confined in a bladder, and raiſed to 104. A pint of water was taken at 54. The bladder containing the air being immerſed in the water, the temperature of the water at the end of

	ſurface		bottom	
1 minute was	54	—	54	
2	54¼	nearly,	54¼	nearly
3	54¼	—	54¼	

The bladder in which this air was contained being freed from air, and raiſed 50 degrees above the temperature of a pint of water, communicated to the water, one fourth of a degree.

This experiment, was frequently repeated with the ſame reſult.

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EXPERIMENT V.

Air in the room 67. A pint of air, elevated from rofin by heat, was confined in a bladder, and was raifed to 104.

A pint of water was taken at 64. The bladder containing the air, being immerfed in the water, the temperature at the end of

			ſurface	bottom
1 minute was		—	64½	64
2	—	—	64½	64
3	—	—	64½	64
4	—	—	64¾	64¼

The bladder in which the air was contained, being freed from air, communicated to a pint of water in the fame circumftances, half a degree of heat.

To obtain this air, a fmall quantity of rofin, was put into a gun barrel, the end of which was heated red hot. When the rofin was violently inflamed, frefh air was blown into the touch hole, and the fumes immediately beginning to iffue copioufly, a flaccid bladder was fixed upon the end of the barrel.

The air which was thus obtained, was partly inflammable; when it was forced upon a candle, it burned with a pale bluifh flame. It confifted chiefly of fixed and phlogifticated air.

EXPERIMENT VI.

A pint of air elevated from tallow, as in the former experiment, and confined in a bladder, was raifed to 113.

A pint of water was taken at 63. The bladder containing the air being immerfed in the water, the temperature, at the end of

1 mi-

		surface		bottom
1 minute was		63¾	—	63
2	—	63½	—	63¼
3	—	63½	—	63½
4	—	63½	—	63½

The bladder being then freed from air, and immersed in a pint of water, in the same circumstances, raised the water half a degree.

This air was also partly inflammable, but consisted chiefly of fixed and phlogisticated air.

From these experiments it appears, that the quantity of sensible heat communicated by fixed and phlogisticated air, to an equal bulk of water, (the difference of temperature being 50) is so small, that it cannot be measured by the thermometer. But we have seen, that in the same circumstances, a pint of atmospherical air communicates one degree of heat to a pint of water. From hence we may conclude, with certainty, that the absolute heat of atmospherical air is greater than that of fixed or phlogisticated air.

The specific gravity of fixed air was found by the honourable Mr. Cavendish, to be to that of water, as 1 to 511. If, therefore, it had contained the same absolute heat with water, it would have communicated to the water nearly the eleventh part of a degree. If it had contained less absolute heat than water, it would have communicated less than the eleventh part of a degree. But such minute variations of heat cannot be distinguished by the thermometer; and, therefore, from this experiment, we can draw no precise conclusion, with regard to the comparative heat of water and fixed air.

It is well known, that a great quantity of fixed air is contained in the crude calcarious earth. Chalk and limestone contain more than a third of their weight of this species of air. It has been proved by

by Dr. Black, that, when theſe ſubſtances are deprived of the fixed air, with which they are combined in their natural ſtate, they are converted into quicklime; and as no ſenſible heat or cold is produced, by the ſeparation of fixed from the calcarious earth (as I ſhall afterwards endeavour to ſhow,) it is poſſible, by comparing the heat of the crude calcarious earth with that of quicklime, to aſcertain, with accuracy, the abſolute heat of fixed air.

The following experiments were made to determine the heat of chalk and quicklime.

EXPERIMENT VII.

A pound of chalk at	—	58,
was mixed with a pound of water at		158;
The temperature of the mixture at the end of		
1 minute was	— —	135,
2	— — —	134.

Adding one degree for the heat carried off by the air in the firſt minute, we have 136 for the temperature of the mixture.

The chalk was raiſed from 58 to 136, or 78 degrees.

The water was cooled by the chalk and the veſſel together 22 degrees. It was cooled nearly 2 degrees by the veſſel—it was, therefore, cooled 20 degrees by the chalk. And hence the heat of water is to that of chalk, as 78 to 20, or as 3.9 to 1.

It is to be obſerved, that no ſenſible heat was produced by mixing equal parts of chalk and water together, when they were both at 57, which was the temperature of the air in the room.

The abſolute heat of quicklime cannot be aſcertained with accuracy, by making water the ſtandard, as theſe ſubſtances, when mixed together, produce much ſenſible heat.

For

For this reafon, I endeavoured to determine the abfolute heat of quicklime, by mixing together equal quantities of chalk and quicklime at different temperatures. But, to determine, whether the experiment could be made with accuracy in this way, I firft made the following experiment on chalk.

EXPERIMENT VIII.

Air	—	—		64.
Half a pound of powdered chalk at				64
was mixed with an equal weight powdered chalk at				164;
The mixture at the end of				
1 minute was		—	—	116
2	—	—	—	114
3	—	—	—	112
4	—	—	—	$111\frac{1}{2}$
5	—	—	—	111
6	—	—	—	$110\frac{1}{4}$
7	—	—	—	$110\frac{1}{4}$
8	—	—	—	110

The veffel in which this experiment was made, was heated to 114, previous to the mixture. Hence it appears, that equal quantities of chalk, at different temperatures, being mixed together; the temperature of the mixture, at the end of two minutes, was half the excefs of the hotter above the colder.

From a variety of trials, I have found that chalk heats and cools very flowly. And this feems to have been the reafon, why the hot and cold chalk were not brought to a common temperature, till the end of the fecond minute.

EXPERIMENT IX.

A pound of quicklime at	—	61
was mixed with a pound of chalk at		161.

The

The mixture at the end of

1 minute was	—	—		110
5	—	—	—	110
6	—	—	—	111
7	—	—	—	111½
8	—	—	—	112
9	—	—	—	111½
10	—	—	—	111
11	—	—	—	110½
12	—	—	—	110

The veſſel was heated to 111, previous to the mixture.

In this experiment we find, that when the chalk and quicklime were mixed together, the thermometer at firſt fell to 110; afterwards it gradually roſe to 112, which it never exceeded; the mixture then cooled at the rate of half a degree in a minute.

If, therefore, we take 112 for the temperature of the mixture, we ſhall probably be very near the truth. At leaſt we ſhall not make the heat of quicklime greater than it really is. For tho' ſome ſenſible heat ſeems to have been produced by the mixture, yet the whole effect which the production of ſenſible heat can have in this calculation, is to diminiſh the heat of quicklime, compared with that of chalk.

Taking 112 for the common temperature, we have 49 for the heat ſeparated from the chalk, and 51 for that gained by the quicklime. And hence the abſolute heat of chalk is to that of quicklime as 51 to 49.

That cold is not produced by the ſeparation of fixed air from the calcarious earth, I endeavoured to ſatisfy myſelf in the following manner.

If cold were produced by the ſeparation of theſe ſubſtances, heat would be produced by their union. This concluſion is not conjectural. It is founded up-

on

on an induction of facts. It is an inference drawn from what in similar cases actually takes place in nature. Thus cold is produced by the evaporation of water, and heat by the condensation of vapour. Like effects have been observed by the ingenious Dr. Black, in a very great variety of natural phenomena. And as no instance can be shown to the contrary, we may safely conclude, in general, that when a body produces cold in consequence of a change of form, it will produce heat when it returns to its former state.

To discover whether sensible heat is produced by the union of quicklime with fixed air, I exposed an ounce and a half of quicklime at 66, to the vapour arising from a mixture of chalk and the vitriolic acid. In a few minutes, the thermometer in the mixture rose to 88; the heat of the vapour was 82; and the thermometer in the quicklime stood at 78.

If sensible heat had been produced, in this experiment, by the union of the quicklime with the fixed air, the heat of the quicklime would have been greater than that of the vapour.

I have also found, that no sensible heat is produced, when quicklime is precipitated from lime water, by fixed air.

We may therefore conclude, with great probability, that sensible heat is not produced by the union of fixed air with the calcarious earth. At least it is certain, from these experiments, and from a variety of phenomena, that if heat is at all produced by the combination of these substances, the quantity is so very inconsiderable, that it cannot affect the conclusions, which are contained in the following pages.

As heat therefore, is not produced by the union of these substances, we may conclude that cold is not produced by their separation, and consequently during this process, they will not absorb heat from the surround-

ſurrounding bodies. Hence it may be inferred, that the quantity of heat contained in the earth and air when ſeparated, is not greater than the heat which they contained previous to their ſeparation.

The following is a brief illuſtration of the truth of this concluſion.

It is found by experiment, that the ſame heat which raiſes the regulus of antimony one degree, will raiſe the calx of antimony only the one third of a degree.

If, therefore, we ſuppoſe that the regulus, when at the common temperature of the atmoſphere, contains 200 degrees of heat, and if we conceive it to be ſuddenly calcined, the heat contained in the regulus, will raiſe the calx, only the one third of 200 degrees, or 66 degrees and $\frac{2}{3}$; the latter will therefore during the calcination abſorb 133 degrees and $\frac{1}{3}$ of heat. And hence the calx is found to contain three times as much abſolute heat as the regulus.

In like manner, if the fixed air and calcarious earth when diſunited, contained a greater quantity of abſolute heat than when combined, the ſeparation of theſe ſubſtances would neceſſarily be attended with the abſorption of heat. But we have proved that no heat is abſorbed during their ſeparation. We may therefore conclude, that, when, a given quantity of chalk, is reſolved into its principles, by converting it into quicklime and fixed air, the abſolute heat of the quicklime and fixed air taken together, is not greater than the heat which was originally contained in the chalk*.

* This concluſion is farther confirmed by the ingenious Dr. Irvine's diſcoveries with regard to the cauſe of the phenomena of latent heat. As theſe diſcoveries, however, have not been communicated to the world, I have not taken the liberty to point out their connection with this part of my ſubject.

From theſe data, the abſolute heat of fixed air compared with that of chalk, may be calculated in in the following manner.

It has been proved that the abſolute heat of chalk is to that of quicklime, as 51 to 49, or nearly as 25 is to 24. We ſhall ſuppoſe that chalk contains one third of its weight of fixed air; and that the abſolute heat, contained in the quicklime and fixed air which are produced by the calcination of a given quantity of chalk, is equal to that which was contained in the chalk, previous to its calcination. If we conceive the whole heat in the chalk divided into 25 equal parts, the heat contained in the quicklime, after the ſeparation of the fixed air, will be to the original heat of the chalk, as 16 to 25. For if the quicklime were equal in quantity to the chalk, its heat would be to that of the chalk as 24 to 25. But as it is only equal to two thirds of the chalk, it will be as 16 to 25. And ſince the heat of the quicklime and fixed air taken together, is equal to the original heat of the chalk, the heat of the fixed air will be equal to the difference between the heat of the chalk and quicklime, or it will be repreſented by the difference between 16 and 25. That is, the heat contained in the fixed air, after the ſeparation, will be to the original heat of the chalk, as 9 to 25, the quantity of matter in the fixed air being one third of that in the chalk. And therefore taking equal quantities of chalk and fixed air, the heat of the fixed air will be to that of the chalk, as 9 multiplied by three, or as 27 to 25, or as $1\frac{2}{27}$ to one.

It has been already ſhown that the heat of chalk is to that of water, as 20 to 78, or as one to 3.9. But the heat of fixed air is to that of chalk as $1\frac{2}{27}$ to one; therefore the heat of fixed air is to that of water, nearly as 1 to 3.6.

From

From theſe principles, we may determine the comparative heat of fixed and atmoſpherical air. The abſolute heat of atmoſpherical air is to that of water, as 18.6 to one. The abſolute heat of water is to that of fixed air as 3.6 to 1. The heat of atmoſpherical air is therefore to that of fixed air, as 18.6 multiplied by 3.6 to 1; or very nearly as 67 to 1.

EXPERIMENT X.

Air in the room — — 52.
Fifteen ounces of water were taken at 51;
A quantity of dephlogiſticated air, equal in bulk to 10 ounces of water, was raiſed to 101;

The bladder containing the air being immerſed in the water, and the ball of the thermometer being kept in contact with the bladder for the firſt two minutes, the temperature at the end of

1 minute was 57,
2 — 55.

The thermometer being then removed from the bladder, the water at the end of

3 minutes was 54,
4 — 54,
5 — 54,
6 — 54:

And this was found to be the heat of the water, at the centre, as well as at the ſurface. The bladder in which this air was contained, communicated to the water, in the ſame circumſtances, the one fourth of a degree, as nearly as could be judged by the eye.

Fifteen ounces of water, being heated in the ſame veſſel, 2 degrees above the temperature of the atmoſphere, cooled, in 20 minutes, 1 degree, or 1 fourth of a degree in 5 minutes. If, therefore, we allow the heat imparted by the bladder, for that

 which

which was carried off by the atmofphere in the firft 5 minutes, we have 3 degrees for the heat communicated to the water, by the dephlogifticated air.

The fpecific gravity of dephlogifticated air, was found, by Dr. Prieftley, to be to that of atmofpherical air as 187 to 185. Its fpecific gravity is confequently to that of water, nearly as 1 to 852. But the bulk of the water, in the above experiment, was one third greater than that of the air. Since, therefore, if equal bulks had been taken, the water would have been to the air as 852 to 1; it follows, that as the water was one third greater in bulk than the air, the quantities of matter were as 1278 to 1, The heat received by the veffel was equal to that which would have been received by one ounce of water. The water and veffel together were therefore equal to 16 ounces of water; and the quantity of matter in 16 ounces of water being to that contained in 10 ounce meafures of dephlogifticated air, as 1363 to 1, it follows, that the abfolute heat of dephlogifticated air, is to that of water in the compound ratio of 1363 to 1, and of 3 to 47, or, as 87 to 1.

To obtain this air, a quantity of red lead was moiftened with yellow fpirit of nitre, and the falt being dried and put into a glafs veffel, the air was feparated by an intenfe heat, and caught in bladders. It appeared to be of a very pure kind, as a candle burned in it with a crackling noife, and with a bright and vivid flame.

From this experiment, compared with experiment the 1ft, it appears, that the abfolute heat of dephlogifticated air, is to that of atmofpherical air, as 87 to 18.6, or nearly as 4.6 to 1. And Dr. Prieftley, whofe difcoveries on this fubject are defervedly much admired, has proved that its power in fupport-

ing

ing animal life, is 5 times as great as that of atmofpherical air.

We have, therefore, upon the whole, fufficient evidence for concluding, that atmofpherical air contains a greater quantity of abfolute heat, than the air which is expired from the lungs of animals; and that the quantity of abfolute heat contained in any kind of air that is fit for refpiration, is very nearly in proportion to its purity, or to its power in fupporting animal life.

PROPOSITION II.

THE blood which paffes from the lungs to the heart, by the pulmonary vein, contains more abfolute heat, than that which paffes from the heart to the lungs, by the pulmonary artery.

As the former is the blood which is returned by the veins in the aortic fyftem, and the latter is that, which, in the fame fyftem, is propelled into the arteries, I fhall call the firft venous, and the laft arterial blood.

The following experiments were made to determine the heat of venous and arterial blood.

EXPERIMENT I.

Air in the room — — 68

Half a pound of water, averdupoife weight, at — — — 53,

was mixed with half a pound and 400 grains of arterial blood at — 102.

The mixture at the end of

1 minute was 78,

2 — $77\frac{3}{4}$ nearly,

3 — $77\frac{1}{2}$ when it coagulated,

4 — $77\frac{1}{2}$

EXPE-

EXPERIMENT II.

Half a pound of water, averdupoife weight,
at — — $53\frac{1}{2}$
was mixed with nine ounces and a half, and
14 grains of venous blood, at $99\frac{1}{3}$
The mixture at the end of
1 minute was 76,
$3\frac{1}{2}$ — 76 when it coagulated,
8 — 76,
9 — $75\frac{3}{4}$.

In making thefe experiments, it was neceffary to ufe as much expedition as poffible, that the heat of the mixture might be determined previous to the coagulation; and, therefore, the water was firft accurately weighed.—Half a pint of blood was taken from the carotid artery of a fheep, for the firft experiment, and from the jugular vein for the fecond: the heat of the mixture was then afcertained by the thermometer, and the weight of the blood was determined at the conclufion of the experiment.

We learn from thefe experiments, that the fpecific gravity of venous blood is greater than that of the arterial. For the meafures were nearly equal, and the weight of the former was found confiderably to exceed that of the latter. The arterial blood appeared alfo to be much more fluid than the venous; and we have feen, that when they were mixed with equal quantities of water, the venous blood was fomewhat later in coagulating, than the arterial.

To determine the heat of arterial blood, from the former of the above experiments, we may obferve, that as, in this experiment, the blood was poured upon the water, a fmall portion of heat was loft in its paffage through the air. I have found by a fubfequent trial, that the quantity of heat which was thus loft, was very nearly one degree. If this heat had been added to the mixture, it would have raifed

it

it nearly half a degree; and as the mixture, previous to its coagulation, cooled at the rate of one fourth of a degree in a minute, we may add, at leaſt, half a degree for the heat loſt in the firſt minute; which gives 78½ for the temperature of the mixture.

This experiment was made in a pint pewter veſſel, the capacity of which, for receiving heat, was to that of the water, nearly as 16 to 1. The quantity of water was eight ounces; the heat received by the veſſel, was conſequently equal to that which would have been received by the one-ſixteenth part of eight ounces, or by half an ounce of water. It follows, that the effect of the veſſel and the water together, was equal to that which would have been produced by eight ounces and a half of water.

The temperature of the mixture was 78½: ſubtracting this from 102, we have 23½ for the heat ſeparated from the blood. The water and the veſſel were raiſed from 53 to 78½, or 25½. The quantity of blood was eight ounces and 400 grains averdupoiſe, or 3899 grains. The water and the veſſel together were equal to eight ounces and a half of water, or to 3717 grains. And, therefore, the heat of arterial blood is to that of water, in the compound ratio of eight ounces and a half to eight ounces and 400 grains, and of twenty-five and a half to twenty-three and a half, or as 103 to 100; conſequently the heat of water is to that of arterial blood, as 100 to 103, or nearly as 97.08 to 100.

In the ſecond experiment, adding half a degree for the heat loſt in the firſt minute, we have 76½ for the temperature of the mixture.

The blood was cooled from 99⅓ to 76½, or nearly 22.83. The water and veſſel were raiſed from 53½ to 76½, or 23 degrees. The quantity of venous blood was 9½ ounces and 14 grains averdupoiſe, or 4168 grains. The water and veſſel were together equal

equal to 8½ ounces of water. Therefore the heat of venous blood is to that of water, in the compound ratio of 8½ ounces to 9½ ounces and 14 grains, and of 22.83 to 23, or as 100 to 112.

Putting A for arterial blood, V for venous, and W for water, the ratio of the heat of venous to that of arterial blood, is determined in the following manner:

	V.	W.	A.
	97.08	100	112.

Therefore V : A :: 97.08 : 112, or nearly as 10 to 11½. Thus it appears, that the blood which paſſes from the heart to the lungs, by the pulmonary artery, contains leſs abſolute heat than that which paſſes from the lungs to the heart by the pulmonary vein.

PROPOSITION III.

THE capacities of bodies for containing heat, are diminiſhed by the addition of phlogiſton, and increaſed by the ſeparation of this principle.

As bodies, when inflamed, appear to emit light, and give out heat, from an internal ſource, and as thoſe bodies only are combuſtible, which contain the phlogiſton in a conſiderable quantity, it has been an opinon generally received among philoſophers, that this principle is either fire itſelf, or intimately connected with the production of fire. If this were true, bodies, when united with phlogiſton, would contain a greater quantity of fire, or of abſolute heat, than when ſeparated from it: metals would contain more abſolute heat than their calces; and ſulphur more than the vitriolic acid. But that the contrary is the fact, as ſtated in the above propoſition, appears from the following experiments.

EXPE-

EXPERIMENT I.

Air in the room	—	64.
Half a pound of water at	—	62,
was mixed with half a pound of tin at		162;

The mixture at the end of

	surface		bottom
1 minute was	68	—	68,
2 —	68	—	68.

EXPERIMENT II.

Half a pound of water at — 62,
was mixed with half a pound of the grey calx of tin at — — 162;
The mixture at the end of

	surface		bottom		medium
1 minute was	68	—	72	—	70,
2 —	$68\frac{1}{4}$	—	70	—	$69\frac{3}{8}$,
3 —	68[illegible]	—	$69\frac{1}{2}$	—	69,
4 —	69	—	69	—	69.

These experiments were made in a pewter vessel, the capacity of which, for receiving heat, was determined thus:

Half a pound of water at — 160,
was poured into the vessel at — 60;
The temperature of the water at the end of

1 minute was		—	150,
2	—	—	146,
3	—	—	142.

Adding four degrees for the heat carried off by the air in the first minute, we have 154 for the common temperature of the water and the vessel. The absolute heat of the vessel, therefore, is to that of the water, as 6 to 94, or as 1 to $15\frac{2}{3}$. That is, the heat contained in the vessel, was equal to the heat contained in the $\frac{1}{15\frac{2}{3}}$ part of eight ounces of water. Or nearly equal to the heat contained in half an ounce of water.

In the firſt experiment, the tin was cooled 94 degrees, and the water heated 6. Since, therefore, the tin heated eight ounces of water, and the veſſel which was equal to half an ounce of water, ſix degrees, it follows, that it would have heated eight ounces and a half of water ſix degrees. And hence the abſolute heat of tin is to that of water, in the compound ratio of 6 to 94, and of 8.5 to 8, or as 1 to 14.7.

In this calculation, no allowance has been made for the heat which was loſt in the firſt minute. For, though a portion of heat muſt have been ſeparated from the tin by the air, when it was mixed with the water, yet the quantity was ſo very ſmall, that it may be neglected. If one degree of heat were thus ſeparated, it would not have raiſed the mixture more than one-fourteenth part of a degree. And it appears, that, after the tin and water were brought to a common temperature, the mixture cooled ſo very ſlowly, that the heat which was loſt, could not be meaſured by the thermometer. If, however, we were to add one-fourth of a degree for the heat imparted to the air in the firſt minute, the abſolute heat of tin would be to that of water, nearly as 1 to 14.1.

In the ſecond experiment, the mean temperature of the mixture, at the end of one minute, was 70. It cooled nearly at the rate of one-fourth of a degree in a minute. Adding therefore one-fourth of a degree for the heat loſt in the firſt minute, we have $8\frac{1}{4}$ for the heat communicated to the water and the veſſel, and $91\frac{3}{4}$ for that ſeparated from the calx. But the quantity of the calx was eight ounces. The water and the veſſel were together equal to eight ounces and a half of water; and, therefore, the heat of the calx, is to that of water, in the compound ratio of 8.25 to 91.75, and of 8.5 to 8, or as

as 1 to 10.4. Hence the abfolute heat of the calx of tin is to that of tin, as 14.7 to 10.4.

EXPERIMENT III.

A quarter of a pound of water at — 55, was mixed with a quarter of a pound of calx of iron at — — — 155:

The mixture at the end of

	furface		bottom		medium
1 minute was	75	—	77	—	76,
2 —	72	—	76	—	74,
3 —	71	—	74	—	73½
4 —	71½	—	72½	—	72,
5 —	71½	—	71½	—	71½.

During five minutes, the mixture cooled nearly at the rate of a degree in a minute.

Adding one degree for the heat loft in the firft minute, we have 77 for the mean temperature of the mixture. The water, therefore, was heated 22 degrees, and the calx cooled 78.

The quantity of the calx was four ounces. The water and the veffel together were equal to four ounces and a half of water. And, hence, the heat of the calx is to that of water, in the compound ratio of 22 to 78, and of 4.5 to 4; or, nearly as 1 to 3.1.

The heat of iron was found, by Dr. Black, and Dr. Irvine, to be to that of water, as one to eight. I have fince repeated this experiment with nearly the fame refult*.

It appears, therefore, that the abfolute heat of the calx of iron is to that of the metal, as 8 to 3.1.

* I muft farther obferve, that, before I made the experiments which are recited in this fection, the heat of lead and tin, in their metallic ftate, had alfo been determined by the above mentioned philofophers.

EXPERIMENT IV.

Air in the room	—	—	61,
Half a pound of water		—	58,
was mixed with half a pound of lead			158;

The mixture at the end of

		surface		bottom
1	minute was	62½	—	62½,
2	—	62½	—	62½.

EXPERIMENT V.

Air in the room	—	—	61,
One half pound of water		—	60,
was mixed with half a pound of red lead			160;

The mixture at the end of

		surface		bottom		medium
1	minute, was	64½	—	67	—	65¾,
2	—	64½	—	66½	—	65½,
3	—	65	—	66	—	65½,
4	—	65	—	65¼	—	65⅛,
5	—	64½	—	64½	—	64½.

The heat received by the veſſel, in the fourth experiment, was equal to that which would have been received by one half ounce of water. The quantity of water was eight ounces. The water and the veſſel together were equal to eight and a half ounces of water: and, therefore, the quantity of lead being eight ounces, the abſolute heat of lead is to that of water, in the compound ratio of 4.5 to 95.5, and of 8.5 to 8, or nearly as 1 to 19.9.

In experiment fifth, the mean temperature of the mixture, at the end of one minute, was 65¾. During five minutes, it cooled nearly at the rate of one fourth of a degree in a minute. Adding, therefore, one fourth of a degree for the heat loſt in the firſt minute, we have 66 for the temperature of the mixture.

The

The calx was cooled 94; the water and the veſſel were heated 6; and hence the abſolute heat of the calx is to that of water, in the compound ratio of 6 to 94, and of 8.5 to 8, or as 1 to 14.7.

It follows, that the abſolute heat of the calx is to that of lead, as 19.9, to 14.7.

EXPERIMENT VI.

Air in the room — — 58.

One fourth of a pound of water at 59, was mixed with one fourth of a pound of a compound, conſiſting of equal parts of the calces of lead and tin, at 159.

The mixture at the end of

	ſurface		bottom		medium
1 minute was	65	—	69	—	67,
2 —	63	—	67	—	$65\frac{1}{4}$,
3 —	64	—	66	—	65,
4 —	66	—	$65\frac{1}{2}$	—	$65\frac{1}{4}$,
5 —	65	—	$65\frac{1}{4}$	—	$65\frac{1}{8}$,
6 —	65	—	65	—	65.

The mean temperature of the mixture, at the end of one minute, was 67. It cooled two degrees in ſix minutes; adding therefore, one third of a degree for the heat loſt in the firſt minute, we have $67\frac{1}{3}$, or 67.3, nearly for the true temperature of the mixture.

The calx was cooled 91.7. The water heated 8.3. The abſolute heat of the calx therefore is to that of water, in the compound ratio, of 8.3 to 91.7, and of 4.5 to 4, or as 1 to 9.8.

It is to be obſerved that theſe metals calcine more perfectly when mixed, than when ſeparate.

EXPERIMENT VII.

Air — — — 64,

Half a pound of water at — 63,

was mixed with half a pound of the regulus of antimony at 163,

The

The mixture at the end of

1 minute was at the furface and bottom				70,
2	—	—	—	$68\frac{1}{2}$,
3	—	—	—	68.

EXPERIMENT VIII.

Air	—	—	61,
One fourth of a pound of water at			$58\frac{1}{2}$.

was mixed with one fourth of a pound of calx of antimony at $158\frac{1}{2}$.

The mixture at the end of

	furface		bottom		medium
1 minute was	$72\frac{1}{2}$	—	77	—	74
2 —	$71\frac{1}{2}$	—	$74\frac{1}{2}$	—	73
3 —	71	—	72	—	$71\frac{1}{2}$
4 —	$70\frac{3}{4}$	—	71	—	$70\frac{7}{8}$

In Experiment VII. the mixture cooled at the rate of half a degree in a minute. The water and veffel were heated 7.5. The regulus was cooled 92.5. The heat of the regulus, therefore, is to that of water in the compound ratio, of 7.5 to 92.5, and of 8.5 to 8, or nearly as 1 to 11.6. In Experiment VIII. the mean temperature of the mixture was $74\frac{3}{4}$. It cooled in 4 minutes, nearly at the rate of one degree in a minute. Adding therefore one degree for the heat loft in the firft minute, we have $75\frac{3}{4}$ for the true temperature; confequently the abfolute heat of the calx of antimony, is to that of water in the compound ratio of $17\frac{1}{4}$ to $82\frac{3}{4}$, and of $8\frac{1}{2}$ to 8, or nearly as 1 to 4.5, and hence the abfolute heat of the calx of antimony, is to that of the regulus, as 11.6 to 4.5. By fimilar experiments, it may be demonftrated, that the vitriolic acid contains more abfolute heat than fulphur. We may therefore conclude, in general, that bodies, when joined to phlogifton, contain lefs abfolute heat than when feparated from it; and confequently, that, in the former cafe their capacities for contain-

containing heat are diminifhed, and in the latter, increafed.

It follows, that if phlogifton be added to a body, a quantity of the abfolute heat of that body will be extricated; and if the phlogifton be feparated again, an equal quantity of heat will be abforbed.

The calx of antimony, for example, contains nearly three times as much abfolute heat as the regulus: when, therefore, by the addition of phlogifton, the calx is revived, it will lofe two thirds of its abfolute heat, and, on the contrary, when the regulus is by calcination deprived of its phlogifton, the calx will recover the heat which it had formerly loft.

In this point of view, the feparation of heat from a body, by means of phlogifton, and the reabforption of it, when the phlogifton is again difengaged, feems to be analogous to the feparation of air from earths and alkali's, by means of an acid, and the reunion of thefe fubftances with this element, when the acid is feparated. If the vitriolic acid, for inftance, be added to a mild alkali, the fixed air will be extricated, and will fly off in the form of an elaftic vapour; if phlogifton be added to a metallic earth, a portion of the abfolute heat will be feparated, and will fly off in the form of fenfible heat; when the acid is again feparated from the alkali, the latter recovers the air which it had loft, and when the phlogifton is again difengaged from the metallic earth, the earth reabforbs the heat which had formerly efcaped from it.

Heat, therfore, and phlogifton, appear to be two oppofite principles in nature. By the action of heat upon bodies, the force of their attraction to phlogifton is diminifhed; and by the action of phlogifton, a part of the abfolute heat, which exifts in all bodies as an elementary principle, is expelled.

SECT. III.

FROM the facts which have been eſtabliſhed by the above experiments, the following explanation may be given of animal heat, and of the heat which is produced by the inflammation of combuſtible bodies.

I. *Of Animal Heat.*

It has been proved, that the air which is expired from the lungs of animals, contains leſs abſolute heat than that which is inhaled in inſpiration. It has been ſhown, particularly, that, in the proceſs of reſpiration, atmoſpherical air is converted into fixed air; and that the abſolute heat of the former is to that of the latter, as 67 to 1.

Since, therefore, the fixed air which is exhaled by expiration, is found to contain only the one ſixty-ſeventh part of the heat which was contained in the atmoſpherical air, previous to inſpiration, it follows, that the latter muſt neceſſarily depoſit a very great proportion of its abſolute heat in the lungs. It has moreover been ſhown, that the abſolute heat of florid arterial blood, is to that of venous, as $11\frac{1}{2}$ to 10. And hence, as the blood which is returned by the pulmonary vein to the heart, has the quantity of its abſolute heat increaſed, it is evident that it muſt have acquired this heat in its paſſage thro' the lungs. We may conclude, therefore, that, in the proceſs of reſpiration, a quantity of abſolute heat is ſeparated from the air and abſorbed by the blood.

That heat is ſeparated from the air in reſpiration, is farther confirmed by Experiment X. Prop. I. from which experiment, compared with Dr. Prieſtley's diſcoveries, it is manifeſt, that the power of any ſpecies of air in ſupporting animal life, is nearly

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in proportion to the quantity of abſolute heat which it contains, and is conſequently proportionable to the quantity which it is capable of depoſiting in the lungs.

The truth of this concluſion, will perhaps appear in a clearer light, from the following calculation, by which we may form ſome idea of the quantity of heat yielded by atmoſpherical air, when it is converted into fixed air, and alſo of that which is abſorbed, during the converſion of venous into arterial blood.

We have ſeen that the ſame heat, which raiſes atmoſpherical air one degree, will raiſe fixed air nearly 67 degrees. And, conſequently, that the ſame heat, which raiſes atmoſpherical air any given number of degrees, will raiſe fixed air the ſame number of degrees, multiplied by 67. In the Peterſburgh experiment, the heat was diminiſhed 200 degrees below the common temperature of the atmoſphere. We are, therefore, certain that atmoſpherical air, when at the common temperature of the atmoſphere, contains at leaſt 200 degrees of heat. Hence, if a certain quantity of atmoſpherical air, not in contact with any body that would immediately carry off the heat, ſhould ſuddenly be converted into fixed air, the heat which was contained in the former, would raiſe the latter 200 degrees multiplied by 67, or 13400 degrees. And the heat of red hot iron being 1050, it follows, that the quantity of heat, which is yielded by atmoſpherical air, when it is converted into fixed air, is ſuch, (if it were not diſſipated) as would raiſe the air ſo changed to more than 12 times the heat of red hot iron.

If, therefore, the abſolute heat which is diſengaged from the air in reſpiration, were not abſorbed by

the blood, a very great degree of fenfible heat would be produced in the lungs.

Again, it has been proved, that the fame heat which raifes venous blood 115 degrees, will raife arterial only 100 degrees; and, confequently, that the fame heat, which raifes venous blood any given number of degrees, will raife arterial, a lefs number, in the proportion of 100 to 115, or 20 to 23. But we know that venous blood contains at leaft 230 degrees of heat. Hence, if a certain quantity of venous blood, not in contact with any body that would immediately fupply it with heat, fhould fuddenly be converted into arterial, the heat which was contained in the former would raife the latter only $\frac{20}{23}$ of 230 degrees, or 200 degrees; and confequently the fenfible heat would fuffer a diminution, equal to the difference between 230 and 200, or 30 degrees. But the common temperature of blood is 96; when, therefore, venous blood is converted into arterial in the lungs, if it were not fupplied by the air, with a quantity of heat proportionable to the change which it undergoes, its fenfible heat would be diminifhed 30 degrees, or it would fall from 96 to 66.

That a quantity of heat is detached from the air and communicated to the blood in refpiration, is moreover fupported by the experiments which have been brought in proof of the third Propofition; from which it appears, that, when bodies are joined to phlogifton, they lofe a portion of their abfolute heat, and that when the phlogifton is again difengaged, they reabforb an equal portion of heat, from the furrounding bodies.

Now it has been demonftrated, by Dr. Prieftley, that, in refpiration, phlogifton is feparated from the blood and combined with the air. During this procefs, therefore, a quantity of abfolute heat muft necef-

neceffarily be difengaged from the air, by the action of the phlogifton; the blood, at the fame moment, being left at liberty to unite with that portion of heat, which the air had depofited,

And hence animal heat feems to depend upon a procefs, fimilar to a chemical elective attraction. The air is received into the lungs, containing a great quantity of abfolute heat, The blood is returned from the extremities, highly impregnated with phlogifton. The attraction of the air to phlogifton, is greater than that of the blood. This principle will, therefore, leave the blood to combine with the air. By the addition of the phlogifton, the air is obliged to depofit a part of its abfolute heat; and as the capacity of the blood is at the fame moment increafed by the feparation of the phlogifton, it will inftantly unite with that portion of heat which had been detached from the air.

We learn from Dr. Prieftley's experiments, with refpect to refpiration, that arterial blood has a ftrong attraction to phlogifton: It will confequently, during the circulation, imbibe this principle from thofe parts which retain it with leaft force, or from the putrefcent parts of the fyftem: And hence the venous blood, when it returns to the lungs, is found to be highly impregnated with phlogifton. By this impregnation, its capacity for containing heat is diminifhed. In proportion, therefore, as the blood which had been dephlogifticated by the procefs of refpiration, becomes again combined with phlogifton, in the courfe of the circulation, it will gradually give out that heat which it had received in the lungs, and diffufe it over the whole fyftem.

Thus it appears, that, in refpiration, the blood is continually difcharging phlogifton and abforbing heat; and that in the courfe of the circulation, it

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is continually imbibing phlogiſton and emitting heat.

It may be proper to add, that, as the blood by its impregnation with phlogiſton, has its capacity for containing heat diminiſhed; ſo, on the contrary, thoſe parts of the ſyſtem from which it receives this principle, will have their capacity for containing heat increaſed, and will conſequently abſorb heat.

Now, if the changes in the capacities, and the quantities of matter changed in a given time were ſuch, that the whole of the abſolute heat ſeparated from the blood were abſorbed, it is manifeſt, that no part of the heat, which is received in the lungs, would become ſenſible in the courſe of the circulation.

That this, however, is not the caſe, will, I think, be evident, from the following conſiderations.

We know that ſenſible heat is produced by the circulation of the blood; and we have proved by experiment, that a quantity of abſolute heat is communicated to that fluid in the lungs, and is again diſengaged from it, in its progreſs thro' the ſyſtem. If, therefore, the whole of the abſolute heat, which is ſeparated from the blood, were abſorbed by thoſe parts of the ſyſtem from which it receives the phlogiſton, it would be neceſſary to have recourſe to ſome other cauſe, to account for the ſenſible heat which is produced in the circulation. But, by the rules of philoſophiſing, we are to admit no more cauſes of natural things, than ſuch as are both true, and ſufficient to explain the appearances; for nature delights in ſimplicity, and affects not the pomp of ſuperfluous cauſes*.

We may, therefore, ſafely conclude, that the abſolute heat which is ſeparated from the air in reſpiration,

* See Newton's Principia b. iii. p. 202.

tion, and abſorbed by the blood, is the true cauſe of animal heat.

It muſt nevertheleſs be granted, that thoſe parts of the ſyſtem which communicate phlogiſton to the blood, will have their capacity for containing heat increaſed; and therefore, that a part of the abſolute heat which is ſeparated from the blood will be abſorbed.

But from the quantity of heat, which becomes ſenſible in the courſe of the circulation, it is manifeſt that the portion of heat which is thus abſorbed, is very inconſiderable.

It appears, therefore, that the blood, in its progreſs thro' the ſyſtem, gives out the heat which it had received from the air in the lungs; a ſmall portion of this heat is abſorbed by thoſe particles which impart the phlogiſton to the blood; the reſt becomes redundant, or is converted into moving and ſenſible heat.

I ſhall hereafter ſhow, that the heat which is produced by this proceſs, is ſimilar to that which is produced by the inflammation of combuſtible bodies, with this difference, that, in the latter inſtance, the fire is ſeparated from the air, in the former, from the blood.

Of the Inflammation of combuſtible Bodies.

From the above experiments we learn, that atmoſpherical air contains much abſolute heat; that when it is converted into fixed and phlogiſticated air, the greater part of this heat is detached; and that the capacities of bodies for containing heat, are diminiſhed by the addition of phlogiſton, and increaſed by the ſeparation of it. From hence we infer, that the heat which is produced by combuſtion, is derived from the air, and not from the inflammable body.

For

For inflammable bodies abound with phlogiſton, and contain little abſolute heat; atmoſpherical air, on the contrary, abounds with abſolute heat, and contains little phlogiſton. In the proceſs of inflammation, the phlogiſton is ſeparated from the inflammable body, and combined with the air; the air is converted into fixed and phlogiſticated air, and at the ſame time gives off a very great proportion of its abſolute heat, which, when extricated ſuddenly, burſts forth into flame, and produces an intenſe degree of ſenſible heat. We have found by calculation, that the heat which is produced by the converſion of atmoſpherical into fixed air, is ſuch, if it were not diſſipated, as would be ſufficient to raiſe the air ſo changed, to more than twelve times the heat of red hot iron. It appears, therefore, that in the proceſs of inflammation, a very great quantity of heat is derived from the air.

It is manifeſt on the contrary, that no part of the heat, can be derived from the combuſtible body. For the combuſtible body during the inflammation, being deprived of its phlogiſton, undergoes a change ſimilar to that which is produced in the blood, by the proceſs of reſpiration; in conſequence of which, its capacity for containing heat is increaſed. It, therefore, will not give off any part of its abſolute heat, but, like the blood in its paſſage thro' the lungs, it will abſorb heat.

The calx of iron, for example, is found to contain more than twice as much abſolute heat, as the iron in its metallic form; from which it follows, that, in the proceſs of inflammation, the former muſt neceſſarily abſorb a quantity of heat, equal to the exceſs of its heat above that of the latter. Now, from whence does it receive this heat? It cannot receive it from the iron. For the quantity of heat in the

the calx, is more than double of that which was contained in the iron, previous to the calcination.

But in the burning of iron, the phlogiston is separated from the metal, and combined with the air; and it has been proved, that, by the combination of phlogiston with air, a very intense heat is produced. From hence it is manifest, that, in the inflammation of iron, the atmospherical air is decompofed, a very great proportion of its absolute heat is separated, part of which is absorbed by the calx, and the rest appears in the form of flame, or becomes moving and sensible heat.

We may conclude, therefore, that the sensible heat which is excited in combustion, depends upon the separation of absolute heat from the air by the action of phlogiston.

In confirmation of this conclusion, it may be proper to add, that, (if we except the change that the air undergoes in the process of respiration, in which the heat is absorbed) the sudden conversion of atmospherical, into fixed and phlogisticated air, is invariably accompanied with the production of sensible heat. Thus sensible heat is produced when common air is mixed with nitrous air, when it is exploded with inflammable air, when it is diminished and rendered noxious, by putrefaction, by combustion, and by the electric spark. If the quantity of air which is changed, by these processes, in a given time, be very great, the change is attended with much light, with a vivid flame, and with intense heat; but if the alteration in the air be slow and gradual, the heat passes off imperceptibly to the surrounding bodies.

It appears, upon the whole, that atmospherical air contains, in its composition, a great quantity of fire or of absolute heat. By the separation of a portion of this fire in the lungs, it supports the temperature

perature of the arterial blood, and this communicates that *pabulum vitæ*, which is ſo eſſential to the preſervation of the animal kingdom. And, finally, by a ſimilar proceſs, it maintains thoſe natural and artificial fires, which are excited by the inflammation of combuſtible bodies.

Aſſuming this doctrine as true, I ſhall next endeavour to ſhew, that it affords an eaſy ſolution of the moſt remarkable facts, relating to animal heat and combuſtion.

SECT. IV.

Of the principal Facts relating to Animal Heat.

1. THE above doctrine explains the reaſon why the breathing animals have a higher temperature than thoſe which are not furniſhed with reſpiratory organs; for it has been proved that the former are continually abſorbing heat from the air: And, it is probable, that, to provide an apparatus for the abſorption of heat, was the chief purpoſe of nature, in giving to ſo great a part of the animal creation, a pulmonary ſyſtem, and a double circulation.

We have ſhown that animal heat, and the inflammation of combuſtible bodies, depend upon the ſame cauſe, that is, upon the ſeparation of abſolute heat from the air, by the action of phlogiſton.

The quantity of air phlogiſticated by a man in a minute, is found, by experiment, to be equal to that which is phlogiſticated by a candle, in the ſame ſpace of time. And hence a man is continually deriving as much heat from the air, as is produced by the burning of a candle.

It is remarked by naturaliſts, that the cold animals have alſo the power of keeping themſelves at a temperature,

perature, ſomewhat higher than the ſurrounding medium. To account for this, we may obſerve, that animal heat depends, indirectly, upon a change produced in the air by reſpiration, and directly upon a change which the blood undergoes in the courſe of the circulation.

In conſequence of the tendency of the ſyſtem to putrefaction, the blood is impregnated with phlogiſton, and by this impregnation is obliged to give out a part of its abſolute heat. The ſource from which it is again ſupplied with heat, in ſuch of the cold animals as are not furniſhed with lungs, can only be determined by experiment. It is probable, that, in thoſe animals, the aliment contains more abſolute heat than the blood. If this be the caſe, the blood will be ſupplied with heat from the aliment.

2. From the experiments in heated rooms, it appears, that the animal body has, in certain ſituations, the power of producing cold, or of keeping itſelf at a lower temperature than the ſurrounding medium.

This power has been attributed by ſome philoſophers to the evaporation from the ſurface of the body; and indeed it muſt be allowed, that the increaſed evaporation, will have a very conſiderable influence in diminiſhing the heat. But the experiments, which have been related above, point out another cauſe, which, I apprehend, conſpires in producing the ſame effect.

By the heat of the ſurrounding medium, the evaporation from the lungs is increaſed. Now it may be ſhown, that if the evaporation from the lungs be increaſed to a certain degree, the whole heat which is ſeparated from the air, will be abſorbed by the aqueous vapour.

 From

From the calculation in Sect. III. page 58, it appears, that the capacity of the blood for containing heat, is ſo much increaſed in the lungs, that if its temperature were not ſupported by the heat which is ſeparated from the air, in the proceſs of reſpiration, it would fall from 96 to 66. Hence if the evaporation from the lungs be increaſed to ſuch a degree, as to carry off the whole of the heat that is detached from the air, the arterial blood, when it returns by the pulmonary vein, will have its ſenſible heat greatly diminiſhed, and will, conſequently, abſorb heat from the veſſels which are in contact with it, and from the parts adjacent. And thus the very ſame proceſs which formerly ſupplied the animal with heat, will now become the inſtrument of producing cold, and the quantity of cold produced, will be in proportion to the velocity of the blood through the lungs, and the fulneſs and frequency of the reſpiration.

Hence alſo we may perceive the reaſon, why the heat of animals is nearly the ſame, in all parts of the earth, notwithſtanding the very great variations in the heat of the atmoſphere, ariſing from the viciſſitudes of the weather, and the difference of ſeaſon and climate.

The quantities of heat loſt by bodies, when heated and placed in the cold air, are in proportion to the exceſs of their ſenſible heat, above that of the ſurrounding atmoſphere. The heat of the human body is very nearly, at all ſeaſons of the year, 96; and, conſequently, other circumſtances continuing the ſame, the quantity of heat loſt in a given time, when the air is at 36, will greatly exceed that which is loſt in an equal portion of time, when it is at 66. It is therefore, neceſſary, that, in the former caſe, a greater quantity of heat ſhould be abſorbed from the air to ſupply the waſte.

To

To account for this, we may obferve, that, by the tonic and ftimulant powers of cold, the vigour of the animal body is increafed. The veffels on the furface are conftricted. The blood is determined to the lungs. The pulfe and the refpiration are rendered full and frequent. And hence, as the cold advances in winter feafons, and in northern climates, the quantity of heat abforbed from the air is proportionably increafed.

In fummer, the blood is determined to the furface, the velocity of the circulation through the lungs, is diminifhed, and hence, a proportionable diminution in the quantity of heat abforbed. We may add to this, that, if the quantity of heat abforbed by the vapour, which is exhaled in refpiration, be fo great, as that the remaining portion of the heat depofited by the air, is not fufficient to fupport the temperature of the arterial blood, fome degree of cold will be produced in confequence of the change which the blood undergoes in the lungs.

As animals are continually abforbing heat from the air, if there were not a quantity of heat carried off, equal to that which is abforbed, there would be an accumulation of it in the animal body. The evaporation from the furface, and the cooling power of the air, are the great caufes which prevent this acccumulation. And thefe are alternately increafed and diminifhed, in fuch a manner, as to produce an equal effect. When the cooling power of the air is diminifhed by the fummer heats, the evaporation from the furface is increafed; and when, on the contrary, the cooling power of the air is increafed by the winter colds, the evaporation from the furface is proportionably diminifhed. The influence of this caufe in preferving the equality of

heat in animals, was firft fuggefted to me by my ingenious friend, Mr. Cleghorn.

3 Among different animals, thofe are the hotteft, which breathe the greateft quantity of air in proportion to their bulk; and in the fame animal, the degree of heat is in fome meafure proportionable to the quantity of air inhaled in a given time.

Thefe varieties appear to be the neceffary confequence of the general fact, that the heat of the breathing animals is derived from the air. For if animal heat depends upon a change which the air undergoes in the lungs, it is evident, that, all other circumftances being equal, the greater the quantity of air which is changed in a given time, the greater muft be the heat produced.

In exercife, by the action of the mufcles, the venous blood is returned in greater quantities than ufual, from the extremities, to the right auricle of the heart. By the action of the heart it is determined to the lungs. The refpiration is accelerated; the velocity of the circulation is increafed; and hence, a proportionable increafe in the quantity of phlogifton difcharged, and the quantity of heat abforbed.

The cold ftage of fevers is preceded by langour, a fenfe of debility, and a diminution in the action of the heart and arteries. The refpiration is fmall, the pulfe is weaker than natural—the quantity of blood which paffes thro' the lungs, in a given time, is diminifhed—and hence, lefs phlogifton will be difcharged from the blood, and, confequently, lefs heat will be feparated from the air.

In the progrefs of the cold ftage, a fpafm is formed upon the furface*. By the conftriction of the veffels on the furface, the blood is determined to the heart. The heart is ftimulated to more frequent and

* See Cullen's firft Lines of the Practice of Phyfick.

and violent contractions. The velocity of the blood through the lungs is increafed——the refpiration is accelerated; and hence a greater quantity of heat will be abforbed.

We may obferve, that the abforption of heat, and the accelerated velocity of the blood through the lungs, will act and react upon each other, in fuch a manner, as that the heat will have a conftant tendency to increafe. For the accelerated velocity of the blood occafions a greater abforption of heat; and the increafed abforption of heat, by ftimulating the heart and arteries to more frequent and powerful contractions, will again accelerate the velocity of the blood, which will ftill farther increafe the abforption. And therefore, the heat will continue to be accumulated, till counteracted by the operation of fome other caufe. From the fudden diminution in the weight of the body, notwithftanding the quantity of watery fluids that are taken in, and the obftruction of the urinary fecretion, it appears, that, in the hot ftage of fevers, there is a very great evaporation from the furface; we may, therefore, conclude, that this is one of the means which Nature employs, for moderating the heat, and reftraining the violence of the difeafe.

Another caufe which prevents the accumulation of heat, is the cooling power of the external air. We have already obferved, that the quantities of heat loft by a body in a given time, are in proportion to the excefs of its heat, above that of the furrounding medium.

If, therefore, the fenfible heat of the body increafe, while the temperature of the air continues the fame, the quantities of heat carried off by the latter, in a given time, will be proportionably increafed.

In putrid fevers, to the accelerated velocity of the blood through the lungs, there is added a putrefcent

ftate

ſtate of the ſyſtem; in conſequence of which, the air inhaled in inſpiration, will be more copiouſly ſupplied with phlogiſton, than when the body is in a ſound and healthy ſtate. If, in the latter inſtance, the air which is received into the lungs, were completely ſaturated with phlogiſton, the quantity of heat ſeparated from it, would always be proportionable to the quantity of air inhaled in a given time. But it appears from experiment, that the air which is expired by a healthy animal, is not completely ſaturated with phlogiſton. It is capable of being farther diminiſhed by nitrous air, and not more than the eighth part of it conſiſts of fixed air. The quantity of heat, therefore, which is ſeparated from the air in reſpiration, will be partly in proportion to the quantity of air inſpired, and partly to the quantity of phlogiſton diſcharged from the blood, in a given time.

In fevers of the putrid kind, as the ſolid and fluid parts of the ſyſtem are in a putreſcent ſtate, and, conſequently, retain their phlogiſton with leſs force, a greater quantity of this principle will be diſcharged from the lungs, the air will be more copiouſly ſupplied with it in the proceſs of reſpiration, and will, therefore, impart to the blood a greater proportion of its abſolute heat. To theſe cauſes it is probably owing, that the heat of the human body never riſes ſo high as in putrid fevers.

4. Topical inflammation is accompained with redneſs, with tumour, and with unuſual heat*. From the throbbing of the veſſels, and from microſcopical obſervations, it appears, that the velocity of the blood through the part inflamed, is accelerated; and it is manifeſt, that a tendency to putrefaction muſt be produced by the violent reaction, and by the ſtagnation of the ſerous matter which is ſometimes effuſed into the adjoining cellular texture. It has been already

* See Cullen's firſt Lines of the Practice of Phyſick.

already obferved, that the arterial blood has a ftrong attraction to phlogifton, and that by its union with this principle, in the courfe of the circulation, it is obliged to give out that heat which it had received in the lungs. In the ftate of health, the velocity of the blood through the different parts of the fyftem, and the quantities of phlogifton with which it is fupplied in thofe parts, are adjufted to each other in fuch a manner, that the heat is equally diffufed over the whole. But, if by any irregularity, the balance be deftroyed ; if, by the increafed action of the veffels, the blood be urged with greater violence than ufual through any particular part, or, in confequence of a greater tendency to putrefacton, be more copioufly fupplied with phlogifton, it is manifeft, that a greater quantity of heat will be extricated in that part, in a given time. This heat will ftimulate the veffels into more frequent and forcible contractions, by which the velocity of the blood, and the confequent extrication of heat will be ftill farther increafed. On this principle we may probably account for the partial heats which are produced by topical inflammations, and for thofe which arife in hectic and nervous difeafes.

It will hereafter appear, that the heat is accumulated in topical inflammation, by the increafed velocity of the blood through the part inflamed, in the fame manner as it is accumulated upon the fuel, in combuftion, by directing a ftream of frefh air into the fire.

Of the principal Facts relating to the Inflammation of Combuftible Bodies.

1. We have proved, that the fenfible heat in combuftion is derived from the air, and depends upon the feparation of abfolute heat from this element,

ment, by the action of phlogiſton. From hence it is evident, that when the air, in which an inflamed body is confined, is ſaturated with phlogiſton, and deprived of the greater part of its abſolute heat, the ſource of inflammation will be exhauſted, and the flame will neceſſarily be extinguiſhed. And this explains the reaſon, why a conſtant ſucceſſion of freſh air is neceſſary to inflammation, as well as to the ſupport of animal life.

2. It is highly probable, from Dr. Prieſtley's Experiments, that dephlogiſticated air contains leſs phlogiſton, than any other ſpecies of air. And this concluſion will, I think, be farther confirmed, if we compare *his* Diſcoveries with the Experiments which have been related above.

The ſubſtances from which dephlogiſticated air is obtained, either contain in their natural ſtate very little phlogiſton, or they are ſuch as have the greater part of their phlogiſton ſeparated, by the force of fire, and by the action of the nitrous acid.

We have ſeen that phlogiſton diminiſhes the abſolute heat of bodies; that dephlogiſticated air abounds with abſolute heat; that when a certain quantity of phlogiſton is added to this ſpecies of air, ſo as to reduce it to the ſtate of common air, its abſolute heat is proportionably diminiſhed; and that when a ſtill greater quantity is added, ſo as to convert it into fixed and phlogiſticated air, it is, in a great meaſure, deprived of its abſolute heat. From all which it appears, that dephlogiſticated air contains much abſolute heat, and little phlogiſton. As the ſenſible heat which is produced by combuſtion, depends upon the ſeparation of abſolute heat from the air, by the action of phlogiſton, it is manifeſt that the leſs phlogiſton, and conſequently the more abſolute heat any ſpecies of air contains, the longer it muſt contribute to the ſupport of flame, as well as to the preſervation of

animal

animal life. Agreeably to this conclufion, we find, from Experiments I. and X. Prop. i. Sect. ii. that dephlogifticated air contains nearly five times as much abfolute heat as common air. And Dr. Prieftley has fhown, that five times as much fenfible heat is produced by the converfion of it into fixed and phlogifticated air, as by that of common air; for a candle will continue to burn five times as long in the former, as in the latter fpecies of air: Add to this, that it burns with a much more bright and vivid flame; and as the quantity of phlogifton is increafed, the brightnefs and vivacity of the flame diminifh, till at length the air becomes faturated with this principle, and the flame is extinguifhed.

3. As the fenfible heat, in combuftion, depends upon a change produced in the air, by the phlogifton, which is feparated from the inflammable body; it is manifeft, that (all other circumftances being equal) the intenfity of the heat, muft be in proportion to the quantity of air which is changed in a given time. And hence the heat may be increafed to a very great degree, if a ftream of frefh air be directed upon the fuel, by bellows or by the blow pipe.

We have found, that by the converfion of atmofpherical into fixed air, a quantity of heat is difengaged, which would be fufficient to raife the air fo converted to more than twelve times the heat of red hot iron. And indeed the degree of heat, excited by the inflammation of combuftible bodies, would not be lefs than this, if the fire that is thus extricated, were applied to the fixed air alone, and were to remain in the fame concentrated ftate, in which it is at firft feparated from the atmofpherical air.

But as fenfible heat has a conftant tendency to an equal diffufion, it will inftantly flow from the point inflamed, and fpread itfelf over the furrounding bo-

dies. It will be accumulated upon the fuel, abſorbed by the vapour, and communicated to the atmoſphere: And from the principles which have been eſtabliſhed above, it is manifeſt, that the ſame heat which raiſes fixed air 13400 degrees, would raiſe an equal quantity of atmoſpherial air, only the $\frac{1}{67}$ part of 13400, or 200 degrees. This explains the reaſon, why the heat is ſo intenſe in the flame of a candle, and is ſo greatly diminiſhed at the ſmalleſt diſtance from the flame.

4. Tho' it is highly probable that all bodies have their capacities for containing heat changed in conſequence of the addition or ſeparation of phlogiſton, yet from the experiments which have been recited above, it is manifeſt, that the degree of this change is very different in different bodies. We have ſeen that the capacity of the calx of iron is to that of iron, as 3.1 to 1; that the capacity of minium is to that of lead, as 19.9 to 14.7; that the capacity of the calx of antimony is to that of the regulus, as 11.6 to 3.9; and that the capacity of arterial blood is to that of venous, as $11\frac{1}{2}$ to 10.

It appears, moreover, that different quantities of phlogiſton, are required to ſaturate different bodies. Some of the metals abound more with this principle than others. The quantity of phlogiſton which is required to ſaturate dephlogiſticated air, is much greater than that which will ſaturate common air; and a greater quantity of phlogiſton is required to ſaturate common air, than an equal weight of arterial blood.

From theſe facts it follows, that when phlogiſton paſſes from one body to another, the changes in the capacities of the bodies for containing heat will be different, and unequal quantities of matter will be changed in a given time. Thus, in reſpiration, the phlogiſton is ſeparated from the blood and combin-

ed

ed with the air. The diminution which is produced, by this procefs, in the capacity of the air for containing heat, is greater than the increafe in that of the blood; and as more phlogifton is required to faturate atmofpherical air, than an equal weight of arterial blood, the quantity of the latter which is changed in a given time, will be greater than that of the former.

When two contiguous bodies, at the fame moment, have their capacities for containing heat refpectively increafed and diminifhed, if the changes be fuch, that the whole heat feparated from the one is abforbed by the other, no fenfible heat or cold will be produced.

I fhall proceed to determine the cafes in which this will happen, having firft pointed out fome of the chief circumftances, by which, the fenfible heat of bodies, the capacities for containing heat, and the abfolute heat contained, may be diftinguifhed from each other.

The capacity for containing heat, and the abfolute heat contained, are diftinguifhed as a force from the fubject upon which it operates. When we fpeak of the capacity, we mean a power inherent in the heated body; when we fpeak of the abfolute heat, we mean an unknown principle which is retained in the body, by the operation of this power; and when we fpeak of the fenfible heat, we confider the unknown principle as producing certain effects upon the fenfes and the thermometer.

The capacity for containing heat may continue unchanged, while the abfolute heat is varied without end. If a pound of ice, for example, be fuppofed to retain its folid form, the quantity of its abfolute heat will be altered, by every increafe or diminution of its fenfible heat: but as long as its form continues the fame, its capacity for receiving heat will not be

 affected

affected by an alteration of temperature, and would remain unchanged, though the body were wholly deprived of its heat.

The alterations which are produced in the ſenſible heats of different bodies, by given quantities of abſolute heat, are *greater* or *leſs*, according as the body, to which the heat is applied, has a *leſs* or *greater* capacity for containing heat. Thus it has been proved, that if a quantity of heat be added to a pound of water, which is ſufficient to produce an increaſe in its temperature as 1, the ſame quantity of heat being added to a pound of antimony, will produce an increaſe as 4. The body, therefore, which has the leſs capacity for containing heat, has its temperature more augmented by the addition of a given quantity of abſolute heat, than that which has the greater. Hence the *ſenſible* heat of a body depends partly upon the quantity of its *abſolute heat*, and partly upon the nature of the body in which this heat is contained, and conſequently the *ſenſible heat* may be varied, either by a change in the nature of the body itſelf, or by a change in the quantity of its *abſolute heat*. If the variation of ſenſible heat ariſes from the firſt of theſe circumſtances, it follows, that, in the ſame body, the *ſenſible* heat may vary, though the *abſolute heat* continues the ſame*.

The following proportions obtain, with reſpect to the capacities, the ſenſible heats, and the quantities of abſolute heat.

The capacities of bodies for receiving heat, are conſidered as proportionable to the quantities of abſolute heat which they contain, when the quantities of matter are equal, and the temperature are the ſame. Calling, therefore, the ſenſible heat S, the capa-

* See the obſervations on fixed and atmoſpherical air, venous and arterial blood, page 57.

capacity C, and the abſolute heat A, if the quantities of matter, and the ſenſible heats be given, the capacities will be as the abſolute heats; or if S be given, A will be as C. Again, it has been ſhown, that, in the ſame body, if the form remain unchanged, or, in other words, if the capacity be given, the quantity of abſolute heat will be in proportion to the ſenſible heat. That is, if C be given, A will be as S. Since, therefore, if S be given, A will be as C, and if C be given, A will be as S, it follows that if neither be given, A will be as $S \times C$. Therefore, C will be as $\frac{A}{S}$, and if A be given, C will be reciprocally as S. Conſequently, if the capacities be reciprocally as the ſenſible heats, the quantities of abſolute heat will be equal. Thus, if the capacity of the calx of antimony be to that of the regulus as 3 to 1, and if the ſenſible heat of the former be to that of the latter 1 to 3, the calx and regulus will contain equal quantities of abſolute heat *.

It appears, therefore, that in heterogeneous bodies, if the ſenſible heats be different, the *capacities* may vary, though the *abſolute heat* be the ſame. And it is manifeſt from the foregoing experiments, that, in ſuch bodies, if the capacities be different, the *abſolute heats* may vary, though the *ſenſible heat* be the ſame.

Theſe obſervations being premiſed, the caſes in which no ſenſible heat or cold will be produced, by the paſſage of phlogiſton from one body to another, may be determined in the following manner.

PROPO-

* The ſenſible heat is here ſuppoſed to be computed from the point of total privation

PROPOSITION I.

LET there be two bodies, A and B, which have their capacities for containing heat changed at the ſame moment, the capacity of A being diminiſhed, and that of B increaſed; let the capacity of A before the change, be denoted by C, and after the change, by c; the capacity of B before the change by k, and after the change by K. Then C—c will be the difference of the capacities of A before and after the change; and K—k the difference of the capacities of B. It is affirmed, that, if the temperatures and the quantities of matter changed in a given time, be equal, the differences of the capacities will be as the differences of the abſolute heats.

For the capacities of bodies for receiving heat, are eſtimated (as was obſerved above) by the comparative quantities of abſolute heat which they are found to contain, when the quantities of matter are equal, and the temperatures are the ſame. The temperature and the quantities of matter, therefore, in A and B being equal, the capacities will be directly as the abſolute heats. If the abſolute heat of A be double that of B, the capacity will be double, if triple, triple, &c. And therefore, calling the abſolute heats of A and B before the change, H and p, and after the change h and P, it will be C : c : : H : h. And by converſion C—c : c : : H—h : h. For the ſame reaſon K—k : k : : P—p : p. But becauſe the quantities of matter in A and B are equal, and the bodies before and after the change are conceived to be brought to the ſame temperature, it will be, c : k : : h : p.

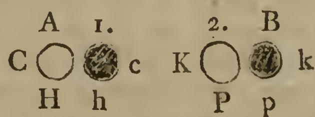
Since

Since therefore C—c : c : k : K—k
H—h : h : p : P—p, by equality
C—c : K—k : : H—h : P—p.

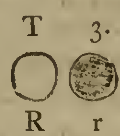
That is, the differences of the capacities are as the differences of the absolute heats, the temperatures and the quantities of matter being equal.

PROPOSITION II.

IF the differences of the capacities be equal, the differences of the absolute heats will be as the quantities of matter*.



The same things remaining as above, if the quantities of matter in A and B be equal, C—c : K—k : : H—h : P—p.



Let the quantitity of matter in A be increased, in any proportion, as in Fig. 3, and, after the increase, let it be called T. Let the absolute heats be denoted by R and r, and the quantities of matter in A (or B) and T, by q and Q respectively.

Since the capacities of A and T are equal, the absolute heats before the change, will be as the quantities of matter. Therefore H : R : : q : Q. For the same reason the absolute heats, after the change, will be as the quantities of matter. That is, h : r : : q : Q. Therefore, H : R : : h : r, and H : h : : R : r, and H—h : h : : R—r : r, and H—h: R—r : : h : r. But h : r : : q : Q. Therefore H—h : R—r : : q : Q. But H—h is equal to P—p. Therefore P—p : R—r : : q : Q. And hence the differences of

* In this and the following proposition, the bodies are supposed to be brought to the same common temperature before and after the change.

of the capacities being equal, the differences of the abſolute heats will be as the quantities of matter.

PROPOSITION III.

IF the differences of the abſolute heats be equal, the differences of the capacities will be reciprocally as the quantities of matter.

For, by the firſt propoſition, the C c quantities of matter being given, the H h differences of the abſolute heats are directly as the differences of the capacities; and, by the ſecond propoſition, the differences of the capacities being given, the differences of the abſolute heats are as the quantities of matter; it follows, that neither being given, the differences of the abſolute heats, are as the differences of the capacities, multiplied into the quantities of matter. That is, H—h is as C—c × Q. Therefore C—c will be as $\frac{H-h}{Q}$; and if H—h be given, C—c will be reciprocally as Q. Conſequently if the differences of the abſolute heats be equal, the differences of the capacities will be reciprocally as the quantities of matter.

Cor. I. It is, therefore, required, in order that neither heat nor cold ſhould be produced, that the differences of the capacities ſhould be reciprocally as the quantities of matter changed in a given time. For in that caſe, by the converſe of this propoſition, the differences of the abſolute heats will be equal.

Cor. II. See the Fig. Prop. I. If, therefore, the diminution in the capacity of A, be to the increaſe in that of B, in a greater proportion, than the quantity of matter in B to that in A, the whole of the heat which is ſeparated from A will not be ab-

ſorbed

ſorbed by B, a part of it will become redundant, or will be converted into moving and ſenſible heat.

Cor. III. The differences of the capacities being reciprocally as the quantities of matter, if the quantities of matter changed in a given time be equal, the differences of the capacities will be equal.

Cor. IV. See the fig. Prop. I. If the quantities of matter changed be equal, and if the heat of A, after the change, be equal to that of B previous to the change, that is, if h be equal to p, in order that neither heat nor cold ſhould be produced, it is required that P ſhould be equal to H. For in that caſe H—h=P—p.

Cor. V. The ſame things being ſuppoſed, if h be leſs than p, then P muſt be greater than H, and P will be equal to H+p—h. For in that caſe H—h will be equal to P—p.

Cor. VI. If h be greater than p, P muſt be leſs than H, and P will be equal to H—h—p. For ſubtracting h—p from H, we have $H-\overline{h+p}=P$. And, therefore, P—p=H—h.

The rule which is expreſſed in the firſt corollary, applies to the ſeparation of heat from the air, and the abſorption of it by the blood, in the proceſs of reſpiration. For as the ſenſible heat in the lungs, is not greater than in the other parts of the body, it is manifeſt, that the whole of the heat which is ſeparated from the air muſt be abſorbed. And therefore the changes produced by the paſſage of the phlogiſton, from the blood to the air, are ſuch, that the difference of the capacities of venous and arterial blood, is to the difference of the capacities of fixed and atmoſpherical air, as the quantity of air changed in a given time, is to that of blood: in which caſe, by the above corollary, no part of the heat will become redundant. The oppoſite changes, therefore, which the air and the blood undergo in

the lungs, precifely balance each other. For as the quantity of blood, which is altered by refpiration, in a given time, is much greater than that of air, fo the change which is produced in the air, during this procefs, is proportionably greater than that which is produced in the blood.

The rule which is expreffed in the fecond corrollary, applies to the fenfible heat produced in the courfe of the circulation, and to that which is produced by the inflammation of combuftible bodies. As we find, for example, that a part of the heat which is difengaged from the blood, in its progrefs through the fyftem, becomes redundant, we may conclude, that the diminution in the capacity of the blood, is to the increafe in the capacity of thofe parts of the body from which it receives the phlogifton, in a greater proportion, than the quantity of matter in the latter to that in the former: in which cafe, the whole of the heat feparated from the blood will not be abforbed; a part of it will be converted into moving and fenfible heat.

That this rule is alfo applicable to the inflammation of combuftible bodies, appears from the following experiments and obfervations.

In the burning of oil, the phlogifton is feparated from its former bafis, and combined with the air. The air is converted into fixed and phlogifticated air—the oil into vapour. By this procefs, the capacity of the air for containing heat is diminifhed, and that of the oil increafed. And, therefore, from the firft and third Corollaries, it follows, that if, in the inflammation of oil, equal quantities of air and oil were changed in a given time, and if the difference of the capacities of oil and the vapour of oil, were equal to the difference of the capacities of fixed and atmofpherical air, the whole heat feparated from the air, would be abforbed by the vapour.

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The difference of the capacities of fixed and atmofpherical air is 66. The capacity of oil is to that of fixed air, nearly as three to one; and, therefore, by the fifth corollary, if the whole heat feparated from the air were abforbed, and if the quantities of air and oil changed in a given time, were equal, the capacity of the vapour of oil would be equal to $67+3-1$. It would be to that of atmofpherical air, as 69 to 67. But we have feen, in the above Experiments, that a pint of atmofpherical air communicates one degree of heat to a pint of water, the difference of temperature being fifty; and that, in the fame circumftances, the quantity of fenfible heat communicated by a pint of the vapour, produced by the inflammation of oil, is fo fmall, that it cannot be meafured by the thermometer.

In the fecond place, fuppofing that atmofpherical air has a greater capacity for containing heat, than the vapour of oil, if the quantity of oil changed in a given time, were proportionably greater than that of air, in this cafe, as appears from the third Propofition, the whole heat feparated from the air, would be abforbed.

The comparative quantities of air and oil, which were changed by combuftion, in a given time, were determined in the following manner:

A candle, weighing nearly three ounces, and burning with a large wick, was found to lofe a fourth of an ounce in twenty-four minutes, or five grains in a minute. Now, if a candle confumes a gallon of air in a minute, and if the one-eighth part of this confifts of fixed air; it follows, that about eight grains of fixed air, will be produced by the burning of a candle, in a minute. From which it appears, that the quantity of air changed in a given time, is much greater than that of oil; and it has been already proved, that the diminution in the capacity of the air for containing heat, is alfo greater than

 the

the increaſe in that of the oil ; we may, therefore, conclude, that only a ſmall part of the heat ſeparated from the air will be abſorbed ; the reſt will be converted into ſenſible heat.

To place this in another light : if, during the inflammation of oil, the whole heat ſeparated from the air, were abſorbed by the vapour, it would follow, that the oily vapour mixed with the fixed and phlogiſticated air, which are produced by the inflammation of a grain of oil, would contain as much abſolute heat, as an equal quantity of atmoſpherical air, mixed with a grain of oil. Now it is well known, that when a candle is ſuffered to burn out in air, the whole maſs of fixed and phlogiſticated air and oily vapour. is contained in leſs ſpace, than the atmoſpherical air, previous to the inflammation. And yet it appears, from Experiment I. and VI. Prop. I. Sect. II that a pint of this compound, contains much leſs abſolute heat than a pint of atmoſpherical air.

I have made ſeveral experiments to determine the quantity of air which is phlogiſticated by the calcination of iron ; and have reaſon to believe, that it is at leaſt equal to the quantity of metal calcined ; from which we may calculate the heat produced by that proceſs.

The capacity of atmoſpherical air is to that of fixed air, as 67 to 1 ; or as 147.4 to 2.2 : The capacity of fixed air is to that of iron, nearly as 2.2 to 1 ; and the capacity of the calx of iron is to that of iron, nearly as 2.5 to 1.

Calling, therefore, the capacity of atmoſpherical air, 147.4, and that of fixed air, 2.2, it appears from Cor. 6. Prop. III. that if the whole heat ſeparated from the air, were abſorbed by the calx, the capacity of the calx would be equal to $147.4-\overline{2.2-1}$ $=146.2$. The heat of the calx would conſequently

be

be to that of the metal as 146.2 to 1. But it is as 2.5 to 1; and hence, the heat which becomes redundant during the calcination of iron, is to the original heat of the iron, as 143.7 to 1; and it is to that of the calx as 143.7 to 2.5, or as 57.4 to 1.

It has been before ſhown, that bodies, when at the common temperature of the atmoſphere, contain at leaſt 200 degrees of heat. During the inflammation of iron, therefore, a quantity of heat becomes redundant, which would be ſufficient to raiſe the calx 200 degrees, multiplied by 57.4, or 11480 degrees. And hence we may account for the heat which is produced by the percuſſion of flint and ſteel. A particle of the metal is ſtruck off by the force of the flint. The phlogiſton is ſeparated from this particle, and is left at liberty to combine with the air, in conſequence of which a quantity of fire is diſengaged from the latter; and the heat which is produced during this proceſs, is ſo intenſe, that the particle of the metal, which is ſtruck off, is converted into glaſs.

Upon the whole, there is ſufficient evidence for concluding, in general, that the heat in combuſtion, depends upon the ſeparation of fire from the air, by the action of phlogiſton; that a part of this fire is abſorbed by the body, which ſupports the inflammation; and that the reſt becomes redundant, or is converted into ſenſible heat. It follows, therefore, that the quantity of fire which is ſeparated from the air, will be in proportion to the quantity of phlogiſton which is joined to it in a given time; and the degree of ſenſible heat which is produced, will be greater or leſs, according as a leſs or a greater quantity of this fire is abſorbed by the body, which diſcharges the phlogiſton.

In the inflammation of alcohol and ſulphur, a very great proportion of the fire which is detached from the air, is imbibed by the aqueous and ſulphureous

reous vapour; and, therefore, alcohol and ſulphur burn with a pale and weak flame. On the other hand, thoſe inflammable bodies which produce little vapour, or which produce a vapour that is capable of abſorbing but little heat, as pit-coal, oil, wax, phoſphorus, burn with a ſtrong and vivid flame; for, in theſe caſes, a great part of the fire which is yielded by the air, is converted into ſenſible heat.

I have thus endeavoured to account for the phenomena of combuſtion and of animal heat, from the general principle, that the capacities of bodies for containing heat, are diminiſhed by the addition of phlogiſton, and increaſed by its ſeparation.

On this principle, a variety of phenomena may be explained, beſides thoſe which have been already mentioned.

When the nitrous acid is mixed with oil of turpentine, the phlogiſton is ſeparated from the oil, and combined with the acid; the latter is forced to give out a portion of its abſolute heat; part of which is abſorbed by the baſis of the oil, and the reſt becomes redundant, or is converted into ſenſible heat. If the ſenſible heat be increaſed to a certain degree, the phlogiſton will ſuddenly combine with the air, in conſequence of which a great quantity of fire will be extricated, and the whole will explode, with a vivid flame, and with intenſe heat.

It is probable, that the vapour of the pure nitrous acid contains as much abſolute heat as atmoſpherical air; for the power of the former in maintaining flame, is nearly as great as that of the latter. In the deflagration of nitre, the acid is converted into vapour; which being at the ſame moment combined with the phlogiſton of the coal, the fire is inſtantly diſengaged, an elaſtic fluid is ſuddenly expanded, and a loud exploſion produced.

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The ingenious Mr. Bewley has given the following explanation of the ſpontaneous accenſion of phoſphorus. He ſuppoſes, with Dr. Prieſtley, that atmoſpherical air contains the nitrous acid, as a conſtituent principle; and obſerving that much heat ariſes from the ſudden combination of phlogiſton, with this acid; he concludes that the phlogiſton of the phoſphorus is capable of decompoſing the air; and that by the union of the phlogiſton with the aerial acid, a degree of heat is produced ſufficient to inflame the phoſphorus.

The experiments which have been recited above, ſeem to prove, as Mr. Bewley has ſuppoſed, that the production of heat is the neceſſary conſequence of the combination of phlogiſton with air: But theſe experiments appear moreover to ſhew, that this heat ariſes from the ſeparation of a quantity of fire, which was contained in the air as a conſtituent principle, and which, in the proceſs of combuſtion, is detached from it, by the action of phlogiſton.

Dr. Prieſtley, has proved that the electric fluid is capable of communicating phlogiſton, to atmoſpherical air, and of converting it into fixed and phlogiſticated air. This fact explains the cauſe of the heat which is produced by the electric ſpark.

If a great quantity of electric matter be ſuddenly diſengaged from a cloud or from the earth, it will extricate a proportionable quantity of fire in its paſſage through the air; and thus we may account for the ſudden riſing of the thermometer in the time of thunder and lightning.

It is found, by experiment, that the phenomena of an earthquake may be imitated by a mixture of iron filings and brimſtone, made into a paſte with water, and buried in the earth. May not the heat

which

which is thus produced be explained in the following manner?

The attraction of the phlogiston to the acid of the sulphur, will be diminished both by the attraction of the iron to this acid, and by that of the water. In the degree of heat which is necessary to the inflammation of sulphur, atmospherical air is capable of separating the phlogiston from the vitriolic acid. Is it not probable that by the assistance of the iron and the water, it may be capable of producing this effect in the common temperature of the atmosphere? If this be the case, it follows, that by the action of the air, which is diffused through the substance of the earth, upon the phlogiston of the sulphur, and by that of the iron and water upon the acid, the sulphur will be decomposed; the air will unite with the phlogiston; the iron with the acid; a quantity of fire will be disengaged from the former, and an inflammable elastic fluid from the latter; and hence a commotion will be excited, accompanied with noise and the eruption of flame, resembling the phenomena of an earthquake.

May not a similar mixture of sulphureous and metallic bodies be produced in consequence of the changes which take place in the bowels of the earth? May not these bodies be brought into contact with the water and the atmospherical air which are diffused through the earth's substance, or lodged in cavities beneath its surface? By the action of the air upon the phlogiston, and of the water and the ore upon the acid, may not the sulphur be decomposed, as in the mixture of iron filings and brimstone? In which case a quantity of fire will be disengaged, and an elastic vapour produced, the latter of which, by its sudden expansion, will excite a commotion in the bowels of the earth, and will at length force its way through the superincumbent strata.

If

If much combuſtible matter be lodged in the regions where the ſubterraneous fires have been kindled, and if this matter be mixed with atmoſpherical air, or with ſubſtances, which, by the application of heat, produce a fluid that is capable of maintaining fire, the inflammation may be augmented to a prodigious degree, and the rarified vapours may carry along with them in their aſcent, a great quantity of ignited materials abounding with phlogiſton, by the expoſure of which, the phlogiſton will be diſcharged, and the flame extended through a large tract of air.

In this manner we may probably account for volcanos, thoſe awful inſtances of combuſtion which are exhibted by nature in the foſſil kingdom.*

It appears, upon the whole, that a variety of important effects are produced in the univerſe in conſequence of the mutual oppoſition of phlogiſton and fire.

Vegetables are elaborated by the aſſiſtance of heat and moiſture, from the elements of earth, air, and water, and by the action of the ſolar light, the principles of which the vegetable tribe is compoſed, are intimately combined with phlogiſton, and are obliged to reſign a portion of their abſolute heat. In combuſtion, the phlogiſton is disjoined from its vegetable baſis, and is combined with the air; and thus thoſe artificial fires are maintained which are ſo neceſſary in the economy of human affairs. In like manner, by the powers of animal life, the phlogiſton is ſeparated from the blood and diſcharged by reſpiration, in conſequence of which a quantity of fire is abſorbed from the air, and is communicated to the animal kingdom.

* The ſame doctrine ſeems to afford an eaſy ſolution of the heat which is produced by fermentation and putrefaction.

The air which was tainted by combuſtion and reſpiration, is again purified by the growth of vegetables †; and if this effect be produced by the ſeparation of phlogiſton, it follows that vegetation will reſtore to the air that heat which had been detached from it in the proceſſes of reſpiration and combuſtion; and thus the principles of phlogiſton and fire, by the medium of atmoſpherical air, will be continually circulating through the animal and vegetable kingdoms.

It may be proper to obſerve, that the doctrine which is advanced in the preceding pages, with reſpect to the cauſe of animal heat and of combuſtion, is the reſult of the general fact, that the changes which are produced in the temperatures of different bodies, by the application of given quantities of heat, are different; or, that, the quantities of matter being equal, the ſame quantity of heat which raiſes *one* body a certain number of degrees, will raiſe *another* a greater or a leſs number, according to the nature of the body to which it is applied. This fact appears to have been ſufficiently verified by experiment; and, therefore, the conſequences which have been deduced from it, muſt, I apprehend, be conſidered as well founded, whatever be the hypotheſis which we adopt concerning the nature of heat.

For this reaſon, I have not entered into the enquiry, which has been ſo much agitated among the Engliſh, the French, and the German philoſophers, whether heat be a *ſubſtance* or a *quality*. It is true, I have, in ſome places, made uſe of expreſſions, which ſeem to favour the former of theſe opinions. But my ſole motive for adopting this language, was becauſe it appeared to be more ſimple and natural, and more conſonant to the facts which had been eſtabliſhed by experiment. At the ſame time, I am per-

† See Dr. Prieſtley's Experiments on Air.

perſuaded, it will be found to be a very difficult matter to reconcile many of the phenomena with the ſuppoſition, that heat is a quality. It is not eaſy to conceive, upon this hypotheſis, how heat can be ſeparated from bodies, by the addition of phlogiſton, or how the abſolute heat can be augmented by the ſeparation of this principle; how the quantity of heat in the air can be diminiſhed, and that in the blood, increaſed, by reſpiration, though no ſenſible heat or cold be produced.

Whereas, if we adopt the opinion, that heat is a diſtinct ſubſtance, or an element *ſui generis*, the phenomena will be found to admit of a ſimple and obvious interpretation, and to be perfectly agreeable to the analogy of nature.

Fire will be conſidered as an elementary principle which enters into the compoſition of all known bodies. In conſequence of the addition of phlogiſton, a portion of the fire will be detached, in the ſame manner as the nitrous acid, is detached, by the vitriolic, from an earth or alkali, and therefore reſpiration and combuſtion will be truly chymical proceſſes, in which, by the exchange of fire and phlogiſton, a double decompoſition will take place, and two new compounds will be formed; the blood, or the inflammable body, parting with phlogiſton and receiving fire, and the atmoſpherical air parting with fire and receiving phlogiſton.

I may add, in the laſt place, that, if fire be conſidered as an element, which is capable of uniting chymically with bodies, a table may be formed, exhibiting the reſpective attractions of phlogiſton and fire.

As phlogiſton ſeparates from bodies, a part of their abſolute heat, it ſhould, be placed at the head of the firſt column: And as we do not know of any ſubſtances, that attract this principle with greater force than the earths of the perfect metals, theſe, perhaps,

M 2 ſhould

ſhould ſtand immediately under phlogiſton. In great degrees of heat, atmoſpherical air ſeparates phlogiſton from all inflammable bodies, and in the common temperature of the atmoſphere, from nitrous air and phoſphorus.

If, therefore, the attractions be arranged as they take place in conſequence of the application of heat, under the earths of the perfect metals ſhould ſtand dephlogiſticated and atmoſpherical air; after which ſhould be placed, the baſes of all the inflammable bodies, diſpoſing them according to the degrees of heat which are neceſſary to their inflammation, when combined with phlogiſton; and at the foot of the column, ſhould ſtand nitrous air.

Fire ſhould be placed at the head of the ſecond column; and if the attractions of bodies to this principle, be proportionable to the quantities of it, which they are found to contain, when the quantities of matter are equal; under fire ſhould ſtand dephlogiſticated and atmoſpherical air—the vapour of the nitrous acid, and probably of ſome other fluids—arterial blood, water, &c.

It is manifeſt, however, that much time, and a ſeries of accurate experiments, would be required in order to the complete inveſtigation of this ſubject.

APPEN-

APPENDIX.

IT may not be amiſs to give the Reader a brief account of the Experiments referred to in page 10, which were inſtituted by Mr. De Luc, with a view to determine the queſtion, whether the thermometer be an accurate meaſure of heat; or, in other words, whether the expanſions of the fluid contained in the thermometer, be in proportion to the quantities of heat applied?

It was laid down as a principle, by this philoſopher, that if equal quantities of the ſame fluid be mixed together at different temperatures, the heat will be equally divided between them, from which it was concluded, that a thermometer being immerſed in the warmer ſubſtance, and alſo in the colder, previous to the mixture, if its expanſions were in proportion to the quantities of heat applied, it would point, after the mixture, to the arithmetical mean, or to half the difference of the ſeparate heats.

Proceeding upon this principle, he found that when a given quantity of water at 32, was mixed with an equal quantity of the ſame fluid at 212, the mercurial thermometer, being immerſed in the mixture, pointed to 180, or to the arithmetical mean between 32 and 212: but the ſpirit of wine thermometer,

mometer, in the ſame circumſtances, did not point to 180. Theſe Experiments were repeated at different temperatures with a ſimilar reſult; from which it was inferred, that alcohol expands irregularly, but that the expanſions of mercury correſpond, preciſely, to the quantities of heat applied.

F I N I S.

Leaves washed and deacidified with magnesium bicarbonate. Resewed. New all-rag end paper signatures. Unbleached linen hinges. Hand sewed headbands. Rebound in $\frac{1}{4}$ Russell's oasis morocco with hand marbled paper sides and vellum corners. Leather treated with potassium lactate and neat's foot oil and lanolin. December, 1979.

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